Philosophy of Science

VOL. 5 July, 1938 NO. 3

CONTENTS

The Scientific Method and Its Extension to Systems of Many Degrees of Freedom. C. H. Prescott, Jr. - - - - - - - - - - - - - - - 237
A Revolution in Metaphysics and in Science. John Elof Boodin - - - - - - - - - - - - 267
Symbolism in Science and Philosophy. Wilbur M. Urban - - - - - - - - - - - - - 276
Nature Common Sense and Science. Eliseo Vivas - - - - - - - - - - - - - - - 300
The Verifiability of Facts and Values. Ray Lepley - 310
Science and the Social Order. Robert K. Merton - - - - - - - - - - - - - - - 321
Science at the Tower of Babel. T. Swann Harding - - - - - - - - - - - - - - - 338
Discussion (Correspondence) - - - - - - - - 354
Reviews and Notes - - - - - - - - - - - - 359

Copyright 1938, The Williams & Wilkins Company

PHILOSOPHY OF SCIENCE

ORGAN OF THE PHILOSOPHY OF SCIENCE ASSOCIATION

The Editorial Board

WILLIAM MARIAS MALISOFF, *Editor*
New York, N. Y.

ERIC TEMPLE BELL
ALBERT E. BLUMBERG
RUDOLF CARNAP
MORRIS RAPHAEL COHEN
W. W. COOK
HERBERT FEIGL
K. S. LASHLEY
HENRY MARGENAU
H. J. MULLER
L. SUSAN STEBBING
DIRK J. STRUIK
ALEXANDER WEINSTEIN

The Advisory Board

EDGAR ALTENBURG
G. D. BIRKHOFF
NIELS BOHR
MARCEL BOLL
P. W. BRIDGMAN
K. T. COMPTON
E. U. CONDON
W. J. CROZIER
JOHN DEWEY
F. G. DONNAN
L. C. DUNN
ALBERT EINSTEIN
R. A. FISHER
J. B. S. HALDANE
R. G. HARRISON
H. HENCKY
H. S. JENNINGS
K. KOFFKA
W. KÖHLER
K. LANDSTEINER
I. LANGMUIR
RALPH LILLIE
A. O. LOVEJOY
W. MAYER
W. P. MONTAGUE
T. H. MORGAN
CHAS. W. MORRIS
JOSEPH NEEDHAM
J. VON NEUMANN
OTTO NEURATH
F. S. C. NORTHROP
H. A. OVERSTREET
M. POLANYI
G. Y. RAINICH
N. RASHEVSKY
H. REICHENBACH
O. L. REISER
O. K. RICE
A. E. RUARK
GEORGE SARTON
C. E. SEASHORE
H. SHAPLEY
E. A. SINGER, JR.
HENRY B. SMITH
W. F. G. SWANN
R. C. TOLMAN
H. UREY
M. S. VALLARTA
PAUL WEISS
GERALD WENDT
HERMANN WEYL
A. N. WHITEHEAD
N. WIENER
SEWALL WRIGHT
F. ZWICKY

Philosophy of Science

VOL. 5 July, 1938 NO. 3

The Scientific Method and Its Extension to Systems of Many Degrees of Freedom

BY

C. H. PRESCOTT, JR.*

INTRODUCTION

WE ARE told that we live in a scientific world. All about us are the fruits of scientific research, and the products of scientific industry. But, in spite of this transformation of our material surroundings, scientific thought, or the scientific method as such, has had no effect upon the everyday thought and behaviour of our people. To be sure, along with the scientific gadgets a few scientific truths have been disseminated. They know the earth is round and moves about the sun; they could know that bacteria cause disease and that they are themselves descended from less articulate forms of animal life. Even when such conclusions are accepted, it is on the basis of authority with no understanding as to how they arose and on what basis they stand. For the large majority, science has merely acquired a cloak of respectability; it is a stronger magic than that of our forebears, and our modern

* Member of the Technical Staff, Bell Telephone Laboratories, Inc.

alchemy is known as a powerful though mysterious source of pecuniary profit.

Perhaps our greatest problem today is to make scientific thought effective in the social and political spheres as it has become in the arts of communication and transportation. But we are more concerned, for the moment, with the reaction of this general ignorance upon scientific thought itself. Due to the increasing demand for the products of applied science and the attendant opportunities for social and economic success, the field is invaded today by the man who wishes to use science but cares not, nor has the leisure, to understand it. With the mores of the general public outside the scientific tradition, and desirous of professional prestige, he learns the authenticated facts of science, acquires the tools of his profession, but never grasps the methods of a continued scientific progress. So, when he turns the last page of his textbook and faces the unknown, too often we witness his retreat to superstitious methods based on faiths and fears of his fetish-worshipping ancestors.

Such a man lacks scientific critique; but what is the scientific method? What can we know? How can we know it? Above all, what is the symbolic relation between this stuff we put in books and the world in which we live? These questions were simpler in the past when the facts were fewer. The truths were as profound, but the field of application was not so broad. It was sufficient to write down the canonical forms of the equations; it was not necessary to express them in generalized coordinates to envelope a modern world.

We were brought up to believe that: (1) Science *is* the codified result of experiment and observation. But this naive belief leads to strange contradictions. To resolve these we learn that, when the results of experiments disagree, it is because the conditions were different.

Next comes the principle of scientific determinism which reads: (2) If the same conditions obtain, an experiment will yield an invariant result. But this principle relegates knowledge to the field of pure speculation, for we can never reproduce the identical

conditions and the results of two experiments never exactly agree.

There emerges the concept of experimental error and scientific truth takes refuge in a method of limits: (3) As we increase the precision of our experiments the errors decrease, and the results must converge to a body of knowledge which is ideal scientific truth. This last concept may correspond to a large body of present scientific opinion. It certainly was the classical notion at the turn of the century when physical research was destined to be the search for further significant figures beyond the decimal point. But do such experimental procedures actually converge? The experience at the turn of the century was that there lurked beyond the decimal point of classical experiment a galaxy of sciences new in scope and theory. (We need only mention X-rays, radioactivity, quantum theory, and relativity.)

So, bewildered by this ingress of the unexpected, we ask again, what is the scientific method, what are the modern criteria of scientific truth, and what are the processes of analytic continuation which will preserve the validity of scientific thought? To attempt to answer these questions is the purpose of this paper, which it is hoped will crystallize those very methods followed, sometimes by tradition, sometimes by intuition, by those workers who do produce valid scientific work.

THE PROBLEM OF CORRELATION

We may infer that scientific knowledge is concerned with the reproducible aspects of nature. It is the repetitive features of observation that permit us to codify past experience, and to predict and to control future occurrences. We may consider a small extension of occurrence which we may experience, or observe, and define it as an event. It may be the presence of a small object, which may be part of a larger structure, or a fragment of a temporal sequence, or "chain of events." We may think of an event as the immediate neighborhood of a point in space-time.

We will find that, while these events are of many kinds, certain

groups of events are associated and their aspects are correlated. Any classification of natural phenomena will be in terms of a quality, or qualities, of events. Such a classification will divide the world into two parts. In the first class will be those events having the quality in question. In the second class will be placed those events which do not have the quality. Thus, in classifying the world according to the quality "A," we will label events "A" or "not A." ("$\overline{A}$").

Now, if we subject the world to an alternate mode of analysis, such as in terms of "B," we will have another classification, "B" and "not B." ($\overline{B}$").

In general, the two modes of analysis will not divide the world in the same way. But if the two classifications should correspond we should say, "There is a correlation between the two qualities. 'A' and 'B'." Such correlations are the essence of scientific knowledge in contrast to the mere description of particular events.

In general the classifications will not exactly correspond, and the world will be divided into four parts: those events having both A and B ($A \odot B$), those having neither A nor B ($\overline{A} \odot \overline{B}$), those having A but not B ($A \odot \overline{B}$), and those having B but not A ($\overline{A} \odot B$). In this simple case we may give the term "correlation" a quantitative significance in several ways. One such expression is the quantity:

$$\frac{(A \cdot B + \overline{A} \cdot \overline{B}) - (A \cdot \overline{B} + \overline{A} \cdot B)}{A \cdot B + \overline{A} \cdot \overline{B} + A \cdot \overline{B} + \overline{A} \cdot B}$$

where the symbols represent the number of events in each group. This form is arbitrary, and one quantity cannot express all the relations of four, but this expression has the advantage of weighting equally all four component classes and of representing antisymmetrically the covariant and controvariant items. Any unsymmetrical expression of the correlation or any weighting which emphasizes a selected group of coincidences leads to statistical fallacies of the positive instance and furnishes a logical method for the establishment of superstition.

QUANTITATIVE CORRELATION

An important case arises when we have two classifications which do not correspond, but do not cross. This will give rise to three regions, $A \odot B$, $\bar{A} \odot B$, and $\bar{A} \odot \bar{B}$. This gives us the opportunity to quantify a quality. Suppose the division lines correspond to the values x_1 and x_2 of the quality x.

In general, to classify events according to an increasing scale of the quality x: let the quality A correspond to the quality x being less than x_1, and the quality B correspond to the quality x being less than x_2. The region $A \odot B$ becomes $x < x_1$; the region $\bar{A} \odot B$ becomes $x_1 < x < x_2$; and the region $\bar{A} \odot \bar{B}$ becomes $x_2 < x$. We may thus set up a sequence of values of x and classify our world into events lying between these various divisions.

The problem of correlation arises again as that of the correspondence between two alternate modes of analysis of our events in terms of two sets of quantities such as the scales of values in x and y. Now each value x_i divides the world into two parts, and likewise each value y_j. For each value of x we can check the correlation with each value of y exactly as in the first case considered. For each value of x we may choose that value of y which gives the largest value to the correlation and call this the value of y corresponding to this value of x.

If, with a great number of events in a finite range of x, we can set up a sequence of a large number of values of x together with a sequence of corresponding values of y, then we shall say that there is a quantitative correlation between the qualities x and y, and the degree of correlation may be expressed as a function of either. If the correlation is unity throughout, then the correlation is perfect and y is simply a mathematical function of x.

But the correlation is usually expressed in a fashion more directly related to the concept of experimental error. We may select the group of events lying between $x - d/2$ and $x + d/2$ and examine the distribution of this group throughout the values of y. The corresponding value of y is given by the mean of this distribution, and the degree of correlation is expressed by some

measure of its dispersion. Again, if the correlation is perfect, the dispersion is zero, and y is a direct function of x.

A few correlations embrace the totality of experience and we may express them in universal terms, but more commonly our observations cover only a restricted region of the world. Again, we may find a correlation which is valid only over a limited range, and is not found or cannot be formulated in other ranges of observation. So, in general, we must state both the region of correlation or observation, whichever is smaller, and the degree of correlation found within that region. Since they are referred to a wider field of experience, these quantities must be expressed in terms independent of the correlation itself. As intimated above, the degree of correlation or the region within which it is formulated may depend upon either of the qualities in a quantitative correlation. But, in general, the definition of the region will depend upon other quantities which are not explicit in the correlation.

Thus, from the totality of events we select a smaller group defined by a certain quality or by a limited range of certain independent properties or variables. Within this region we find that two properties are related more or less closely as expressed in the observed correlation. Such a correlation will express a certain body of experience. If our observations be representative of phenomena within this region, which implies that the definition of the region, or the selection of data, and the observed correlation be truly independent: then other events falling in this region will exhibit the same correlation. When we extend the region of correlation to include the next event, the preexisting correlation measures the probability that the properties considered will be found associated. Such an extrapolation in the temporal extension of the region of correlation is the ultimate basis of scientific prediction and of the technical control of natural phenomena.

Before developing further the concept of correlation we must first consider the idea of correspondence in the sense of functional relation or dependence. This idea is fully developed in elementary mathematics.

FUNCTIONAL CORRESPONDENCE

In the limiting case where the correlation between x and y is perfect, every value of x determines a precise value of y, so we have a single set of number pairs expressing the relation. Mathematically this means that there exists a functional relation between x and y.

$$\phi(x, y) = 0$$

In general, in describing a physico-chemical system, we will have a large number of relations between a larger number of variables.

$$\phi_i(x, y, z, w, \cdots) = 0 \qquad 1 \leq i \leq t$$

However, there will be a lesser number of independent variables describing the condition, or state, of the system, and the other variables will be dependent and determinable from the above relations. We may consider these relations to be solved or reduced to the expression of each of the dependent variables in terms of a suitable set of independent variables. The number of independent variables, or the degree of freedom of the system, is a fundamental property of the system, but the choice of particular independent variables is somewhat arbitrary. However, it is useful to make a distinction between a set of independent variables considered as parameters of the system, sufficient to determine its state, and the dependent variables which then become derived properties of the system. Their dependence on the parameters will be expressed by relations of the form:

$$p_i = f_i(x_1, x_2, \cdots x_n) \qquad 1 \leq i \leq t$$

These are general expressions which in actual cases may be degenerate. A given property may depend on only one or a limited number of the parameters, the others being absent from the function "f." Again, the p's may not be all independent. Also in one of the f's several of the parameters may enter only in some particular combination. Examples are:

$$p_4 = f_4(x_1, x_3)$$
$$\phi(p_1, p_2, p_3) = 0$$
$$p_k = f_k(x_1, x_2 - x_3, x_4/x_5, \cdots x_n)$$

FURTHER CONCEPTS OF SCIENTIFIC CORRELATION

With these in mind, let us consider one metrical property of the system in its relation to the independent parameters, expressing the equivalence of p and f by equating to zero the modulus of their difference.

$$|p - f(x_1, x_2, \cdots x_n)| = 0$$

We have assumed that the relation between p and $x_1 \cdots x_n$ was exact. Actually $x_1 \cdots x_n$, the parameters determining the state of the system, are measured quantities. And the property p is an independently measured quantity.

Thus p and f are two independent classifications which are to be correlated. p is an experimental value of the quality and f is a theoretical value predicted from a corresponding set of measurements. A better expression is:

$$|p - f(x_1, x_2, \cdots x_n)| < \epsilon$$

where ϵ is a finite quantity which measures the discrepancy between the two classifications—one experimental and the other theoretical. But it must be borne in mind that the theory only relates the computed value of f to the experimentally determined x's which constitute a set of independent measurements.

If ϵ is small the correlation is good. If the domain of application is large, the relation is comprehensive. For the purposes of this argument $f(x_1, \cdots x_n)$ may be a quantitative relation predicted by fundamental theory or it may be an empirical relation summarizing past experience. In either case the problem of correlation is the same.

Independent of the accuracy of the measurements, the degree of correspondence will depend upon the correctness of the relation $f(x_1, \cdots x_n)$. As theory and experiment progress they will come into closer agreement. Thus the relation

$$|p - f_j(x_1, \cdots x_n)| < \epsilon$$

has many of the properties of a limit since ϵ decreases with successive approximations as indicated by increasing values of j.

The development of the function "f" will embrace all the problems of "the adjustment of observations" as well as the theoretical development of the problem at hand. From the standpoint of the statistical theory of observations and errors, "ϵ" may assume quantitative significance as the probable error or as the modulus of distribution of the observations. But for the moment our concern is with a more general significance of ϵ which will not depend upon a specific formulation.

We must recall that "f" is assumed to depend on a limited number of parameters, $x_1, \cdots x_n$. But "p" is a phenomenon of the real world whose parameters are beyond reckoning. So we should rewrite the relation:

$$|p(x_1 \cdots x_n \cdots x_\infty) - f_j(x_1, \cdots x_n)| < \epsilon$$

"f_j" again represents the successive stages of approximation as a function of, however, but n parameters. We now see why the relation does not express a true limit and ϵ does not approach zero. "j" may increase without limit, but ϵ remains finite as a measure of deviations in "p" induced by $x_{n+1}, \cdots x_\infty$ which have no representation in "f." ϵ represents a level of achievement to be surpassed only by taking account of an ever increasing number of variables. We may comprehend this relation in the following symbolic formulation:

$$|p(x_1, \cdots x_n, \cdots x_\infty) - f_j(x_1, \cdots x_n)| < \epsilon\,(n)$$

where ϵ is independent of j, i.e. of the development of the function, but depends upon the number of parameters taken into account.

We thus understand why no theory may fully comprehend reality, and that the elegant theorems of elementary science, depending on one to three independent variables, do not lend themselves to a general development of scientific method. Any real phenomenon depends in actuality upon an infinity of parameters. Of necessity each theory is erected on a finite number of independent variables and is valid only to the level of extraneous effects. If we consider this formulation as a Taylor's series in n variables, we may determine the coefficients only till

the remainder becomes less than the perturbations of further variables. So no theory can be exactly confirmed by experiment. The mathematical identification of the function requires an infinity of terms, but the series provided by experiment has a finite remainder. So there are at least an infinite number of mathematical formulations which could represent equally well any experimental data.

We thus see the limitation of theory in dealing with new knowledge and may understand why most discoveries originate in the laboratory through the extrication of new variables. And we see why the attempt to understand fully even one small phenomenon always leads us ultimately to the problem of the universe as a whole in its infinite complexity.

From this mode of analysis our symbolic expression arises as a relation, not necessarily of the world itself, but rather of the mode of human perception and representation—of the scientific method. The fact remains, a fact of pragmatic experience summarized above, that we have elucidated many phenomena to a high degree of accuracy, each in terms of a few parameters. The art of scientific analysis is to extricate the dominant parameters and to ignore those whose effects are either constant or small. If we apply the old principle of William of Occam, we see that n is to be minimized. For a coherent science we *should* select our variables so that n is as small as possible consistent with the attainable size of ϵ.

This yields many different sciences concerned with different groups of parameters and with different domains of application. But as they grow the fields overlap, more general expressions are found, and, at last, each path to knowledge leads back to the essential problem of the universe as a whole. It appears that the procedures of scientific study and measurement do not converge. Instead, a dynamic science with an expanding frontier of knowledge is to be represented, not by a limit, but by an essentially divergent expression.

LOGICAL CONSEQUENCES

In the previous section we have elaborated an expression in mathematical terms to symbolize the structure and evolution of

scientific knowledge. We should note that the development is in no sense a derivation, for not one step follows as a deduction from the one before. Instead, each is an extension of the preceding formulation. The whole discussion is an inductive development of the final symbolic expression. In contrast, the analysis of this section will be the deduction of special principles which may be more immediately applicable to actual problems.

We first note that the substance of scientific knowledge is its established correlations, each a special case of the relation:

$$|p(x_1, \cdots x_\infty) - f_j(x_1, \cdots x_n)| < \epsilon\,(n)$$

where p is the phenomenon studied, f is the formulation which represents it in the universe of discourse, ϵ is the precision with which the correlation holds, and the region of correlation is subsumed in the description of the phenomenon. We do not wish to minimize the importance of theoretical development and mathematical analysis of the function f. Fully recognizing the importance of imagination and intellect in scientific progress, we merely point out that, except for logical deductions from proven principles, theoretical and speculative developments do not become part of the body of science until they have served as the basis for empirical correlation.

This emphasis on correlation leaves rather uncertain the common and valuable notion of "explanation." We have nothing to serve as an explanation except some other correlation. The concept of fundamental, or absolute, explanation disappears into limbo along with the classical ether and the mediaeval dogma of the argument from authority. The term "explanation" occurs as a derivative of the extension of a relation to cover a larger region of correlation. A function f arises as the basis of correlation in a given restricted region. When this same notion f is found to serve as a basis of correlation in another region of investigation: the relation discovered in the first field is the "explanation" of the correlation discovered in the second.

In this sense the second law of thermodynamics which arose in the study of steam engines becomes the "explanation" of the Stefan-Boltzmann law. The law was empirically discovered by Stefan and "explained" by Boltzmann who applied to radiation

the thermodynamic reasoning found previously to hold for steam. Likewise the thermodynamic temperature is "explained" by statistical mechanics as the mean kinetic energy of molecules.

A frequent confusion results from regarding a postulate as the explanation of a theorem which is either logically identical with, or a special case of, the assumed postulate. This results from the assumed direction of the deductive method used for the theoretical presentation of a field of knowledge, once the field is organized and the scaffolding taken down. This is in part a vestigial remnant of the argument from authority which pervaded mediaeval logic. Actually the fundamental postulates are often far from "self-evident truths" but are simply the most convenient or comprehensive statements from which to organize a field of knowledge by logical deduction. The scientific truth consists of the system of correlations comprehended by the logically organized structure of ideal relations.

We shall next try to illuminate the process by which new knowledge is to be acquired. We shall first describe an "ideal manufacturing process." This may be directed to a commercial product or it may be a method to prepare an object for some abstruse laboratory experiment. In fact, it may be any repetitive experiment yielding a quantitative result. We shall be interested in some property "p" of this result. This property will be approximated by a function "f" of certain "n" variables. These will be the manufacturing parameters, some describing the initial state and quantities of materials, and some describing the treatments to which they are subjected.

These parameters, if measured with ultimate accuracy, should determine the property p to within an uncertainty ϵ (n). If we make variations in turn in each of the manufacturing parameters to cause a variation in p relatively larger than ϵ, we may determine the family of partial derivatives $\frac{\partial p}{\partial x_1}$. But these parameters will not be specified as absolute values. Instead each will be allowed to vary over a range given by a specification limit. Now since p is a function of $x_1, \cdots x_n$ etc.

$$\delta p = \frac{\partial p}{\partial x_1} \delta x_1 + \frac{\partial p}{\partial x_2} \delta x_2 \cdots + \frac{\partial p}{\partial x_n} \delta x_n \cdots$$

and we may compute the variation in p to be expected when the process is held within the specification limits $\delta x_1, \cdots \delta x_n$. If we are simultaneously concerned with two or more properties, we will have a circle of diffusion whose dimensions should be determined by the specification limits.

There follows a simple but fundamental principle: If the actual circle of diffusion is markedly greater than that computed from the specification limits, then there is at least one dominant parameter, x_{n+1}, which is not defined by the specifications! Such a situation is not to be remedied by further refinement of existing specifications. The next step is to organize a new attack and to unravel a new variable.

Now no rules can be given for systematic induction; we cannot lay down laws for human intuition and inspiration. But we can determine when a new idea is necessary. So long as each diminution of the specification limits reduces the dispersion of our experimental results, so long may we follow a smooth course of engineering development. But when we reach the irreducible level of ϵ (n) we must discover a new approach. Sometimes a new inspection of old experience will indicate the new direction. Sometimes a resort to sheer rudimentary empiricism, shifting every factor, likely and unlikely in the environment, will uncover an additional source of variation. But to gain any permanent advance requires intellectual thought to recognize and isolate the additional variable. Then, once the new degree of freedom is discovered, we may again apply systematic methods to elucidate its relations and to allocate for it a favorable range of values.

We may once more apply Occam's principle. It is unnecessary and futile to maintain specifications on parameters which make no appreciable contribution to ϵ, the variation permitted in the product. We should also avoid excessively close limits on the controlling variables. If ϵ is set by engineering requirements, we may adopt the rule of thumb common in arithmetical computations: it is reasonable to maintain the specification width of each factor so that its contribution to the variation of the product is one-tenth the total permitted variation. Then the total variation is unlikely to sum to a value outside the limit. If, as is often the case, the limit is set by the precision to which one least

tractable parameter can be held, then the other variables should be so held that their several induced variations are each a factor of ten less than the variation induced by the dominant variable. The precision of the whole process will then correspond to that of this least tractable parameter.

We may set down a few necessary steps in the rationalization of a new field of inquiry. The first is to select a few properties for study and to isolate a group of dominant variables. The second is to work out the form of the relations among them. This should involve the separation of variables, whenever possible, and the determination of the essential degrees of freedom in the relations where the variables are inseparable. Third comes the quantitative evaluation of the relations and a determination of the degrees of correlation. Fourth, when further precision in measurement gives no increase in the accuracy of results, the search is made for another variable to increase the precision of the study or the range of investigation is extended to increase its comprehensiveness.

Technically there correspond stages in the experimental development. First are the duplication of rough experiments to show that the effects are reproducible and a systematic variation of conditions to extricate those factors which cause variations in the results. Second is a rough survey of the field with simultaneous refinement of technical methods to separate the various applied conditions from mutual interference and to obtain a set of specifications each of which, if possible, corresponds to one degree of freedom of the system studied. The third stage is the precise measurement of the relations which may be expressed in mathematical form. Fourth is a recheck of the factors in the ultimate precision of the measurements, and an extension of the technique to adjacent ranges of conditions.

A theoretical interpretation, to be acceptable, must fulfill certain requirements: (1) It must indicate the importance of at least one variable found to be of experimental significance. To be a theory of any status it should be at least qualitatively concerned with a group of variables which control the experimental

situation. (2) A more comprehensive theory will indicate the functional form of the relations. (3) A complete theoretical treatment will not only establish the mathematical form, but will predict the numerical constants of the relations. (4) At each of the previous stages the theory will employ concepts and use constants, when not left for empirical determination, derived from antecedent fields of knowledge.

THE EVOLUTION OF THE FUNCTION "f"

We have so far said little about the function f. This is the symbolic expression by which we summarize experimental results, frequently in mathematical form, often in a theoretical formula. This is the phase of science we know the best, but a few words are fitting regarding the evolution of such an expression.

In the "ideal manufacturing process," the function f is merely a constant. We are concerned with the duplication of a product under one set of conditions for which the value of f is simply the mean of the tests on our previous production. But we have anticipated that p will vary with conditions and shown how to obtain its partial derivatives, which will give the next approximation in the extension of f to cover a wider range of correlation. If we conceive of f as a Taylor's series in n variables we may consider each step, j, of the evolution of the function as the assumption of a longer but finite series of terms and the determination of its constants by comparison with the data.

Now, if we could assume that p were a continuous function of the variables and that the next variable, x_{n+1}, would emerge in an orderly fashion symmetrical with the previous variables, $x_1 \cdots x_n$, we might set up a completely logical sequence of operations. We should add successive terms to the series till the whole region of correlation were comprehended to an irreducible uncertainty ϵ (n). We should then exhume the next variable, x_{n+1}, and proceed as before. So, alternately adding successive terms and variables, we could set up an existence theorem which would converge, though cumbersomely, to a system of absolute knowledge. On the other hand, if we could select n dominant

variables and assume that the others, $x_{n+1} \cdots x_{\infty}$, would contribute but small effects symmetrically distributed at random, we could derive the normal distribution law of errors.

Unfortunately we are all too familiar with molecular phenomena and with the discrete energy changes of atomic theory to expect a mathematical continuity in natural phenomena. We know that our Taylor's series will become inoperable when we reach the level of Brownian movements and resistance noise. We might consider that this is just the point where we are required to introduce a large number of new variables to describe the coordinates and momenta of molecules and electrons. But we should observe that f has become completely inapplicable, and, for this region, the description of our phenomena requires a new formulation ab inito. We should note, however, that most articles of commerce and even physical apparatus are macroscopic systems for which the assumption of continuity will hold far beyond most engineering requirements. There are, thus, vast regions to which, if employed with caution, the concept of a multiple series development will be entirely applicable.

Another difficulty in constructing an existence theorem is the manner in which the next variable may emerge. Sometimes we will pursue a continuous refinement of specifications until we reach the level of the next variable. In another situation a given level of process control may depend upon the fortuitous constancy of some environmental factor; for example, it may depend upon the weather through the influence of temperature or humidity. This will permit long periods of adequate control followed by periods when the process is unworkable. The identification and isolation of the uncontrolled variable should permit the process control to be uniformly maintained, but the incidence of the new variation is a function of time and will be equivalent to the introduction of a systematic error. A similar effect will often occur with a change in the source of some basic material, since a given supply may contain a small but constant proportion of some effective impurity.

The most striking type of incidence of a new variation is a

systematic change in behaviour without precedent or warning and beyond any power to control. Such was the incidence of the "tin disease" in the organ of the castle chapel at Zeitz, reported by Erdmann in 1851.[1] The organ had behaved systematically as a musical instrument for around two hundred years. Then, in the cold of winter, the tin pipes started to change into piles of amorphous powder. Of course metallic tin is unstable below 18°C. and only a seed crystal was needed to initiate the change into the grey tin modification. But no instability could be recognized with respect to an allotropic form not yet known and the origin of the seed crystal is an inscrutable mystery. It is as if the structure of the world had altered to permit a new degree of freedom.

A second case in point was the chance incidence of crystalline glycerine whose melting point is also 18°C. It is said that this was an engineering catastrophe. In the experiments of Gibson and Giauque[2] at Berkeley on the entropy of glycerine, no methods availed to initiate its crystallization. But after some seed crystals were brought into the laboratory from a powder factory in British Columbia, any sample would crystallize on slowly warming from liquid-air temperature, even when great precautions were taken to exclude the presence of seeds.

Any such occurrence marks a discontinuity separating in time two regions of correlation where the identical specifications determine entirely incongruent phenomena. In physics and in chemistry such occurrences are so rare as to have no name. In biology they are more familiar as the mutations responsible for the evolution of new species. In human history they are novel ideas which alter the very structure of society and transform our material surroundings.

We have just discussed discontinuity of the function f where two regions of correlation are separated in the time scale. There are other discontinuities when the range of correlation is extended to include a change of state as at a reproducible melting point.

[1] O. L. Erdmann, Journal für Praktische Chemie 1, 428 (1851).

[2] Gibson and Giauque, Journal of the American Chemical Society 45, 102 (1923).

But there is also discontinuity in f as we increase the precision of correlation in a given region with no alteration in the form or accuracy of the correlation previously obtained.

Up to a certain point a systematic decrease in limits of specification will cause a decrease in ϵ. When neither further decrease in the uncertainties of measurement nor further evolution of *any* function f_j may cause a decrease in ϵ—we have reached the irreducible level ϵ (n). Further progress will entail a radical discontinuity in f—the introduction of another independent variable. But, since we have no systematic method of exhaustion our precision of correlation may be limited, in a practical case, by the assumed form of the function f. It may be initially adequate but fail to conform to some finer structure of the property p. (Such a case arises as we increase the precision of measurement in mapping a sloping roof of corrugated iron, or a wire screen). This will require a discontinuous change in the form of f. There will be necessary at least the introduction of a subordinate function, including at least one additional constant, to describe the fine structure. This is not exactly the introduction of a new variable since the system acquires no new degree of freedom. But the fine-structure constant is a new parameter to be determined for a complete description of the system. In a wider region of correlation this parameter may have a range of values, one of which must be specified to determine each state of the system.

Whether the new parameter is a constant or a variable becomes theoretically a somewhat arbitrary distinction and we may conserve the form of our symbolic expression by defining any such discontinuity in f as an increase in n. In a practical case there is, however, an experimental distinction: An uncontrolled variable will, by definition, introduce a systematic variation shifting with time. But if the new parameter is such that it is held constant (or a function of $x_1 \cdots x_n$) throughout the region of correlation, then the experimental uncertainty will be a function of the as yet unresolved fine structure.

These considerations illustrate the difficulties encountered in establishing systematic correlations in a real world. They are

not reservations in our theory; instead, they illuminate the essential divergence between real phenomena and the ideal relations we use to describe them.

SCIENTIFIC KNOWLEDGE AND PARTICULAR EVENTS

In setting up a scientific method we are concerned with the reproducible aspects of human experience. We develop relations to describe sequences of events which repeat themselves interminably or may be repeated indefinitely by a purposeful control of conditions. But we are confronted by the paradox that no phase of universal history is ever repeated, and, in the universal flow and passage of events, no recurring aspect is ever repeated in the identical setting. Yet the very existence of science, learning, and knowledge presupposes groups of universal aspects, changeless to at least some degree, which are the substance of experience and provide the adaptive purpose of training.

At any instant our universe of reality will have a multitude of metrical properties. Following the method of Willard Gibbs in his statistical mechanics, we may conceive of a phase space of many dimensions in which the coordinates of a point shall represent all the metrical properties of the universe. If we add one more dimension to represent time, the passage of universal history will be described by a world line in this $\infty + 1$ dimensional space.

Ilt shoud be clear that no instantaneous position of our phase point is ever repeated, and no segment of the universal world line is ever congruent with another. Any enduring form or object is a projection of the phase point upon a selected set of axes which remains constant with time. Any repeated experience is a projection of a segment of the world line found congruent with other projections made in a similar way. It is thus that all recognizable forms, and all experiences of special correlations or causal sequences, are mental abstractions from a universal existence and passage.

Such projections are the mode of human approach in the study of nature; such abstractions are the body of human experience; their utility is manifest in the value of knowledge and of scientific method. It is a pragmatic fact of experience that such projective

concepts serve to coordinate past experience and to predict future events. But it is a further fact of experience that the group of variables comprised in a given projection are never completely separable from the rest, so that two projections are never exactly congruent. The search for a higher precision and a wider range of correlation leads always to the inclusion of further variables in the projection. There is always a finite divergence between ideal abstractions and those aspects of phenomena which they serve to represent.

We may return to our symbolic representation of scientific knowledge

$$|p(x_1 \cdots x_\infty) - f_j(x_1 \cdots x_n)| < \epsilon\,(n)$$

p and $x_1 \cdots x_\infty$ are the coordinates of our universal phase point (including time), while p and $x_1 \cdots x_n$ are the coordinates involved in a particular projection. The ideal relation

$$p - f(x_1 \cdots x_n) = 0$$

is the logical, or symbolic, counterpart of the observed phenomena. The quantity ϵ expresses the divergence between this symbolic representation and the group of phenomena which it represents.

Since, in the symbolic relation,

$$p = f(x_1 \cdots x_n)$$

we may say that the symbolic expression $f\,(x_1 \cdots x_n)$ is an ideal function which represents the actual property $p\,(x_1 \cdots x_\infty)$ conceived as a metrical aspect of the universal world line. If from the variables $x_1 \cdots x_n$ the time is missing, our formulation describes the existence through time of a material object, or an ideal form common to many objects. If the time is explicit as one of the variables $x_1 \cdots x_n$, the expression partakes of the universal experience of passage. The formulation describes a repetitive transitive experience of which the symbolic description, $f\,(x_1 \cdots x_n)$, may infer a causal sequence.

Such a term as "symbolic idea" may be used to denote such an enduring form, though we may infer that there is an essential

divergence between the symbolic idea and the natural phenomenon which it represents in experience. For the present discussion it is useful to generalize such an idea to comprehend recurring temporal forms as well. In this we follow the tradition of kinematics which treats time as hardly different from another geometrical dimension. Thus the generalized symbolic idea will refer to any function, f, which serves as a basis for correlation in any department of knowledge.

So far we have been concerned with the induction of correlations. Taking a large group of occurrences, arrangements or sequences of events, we abstract the pattern of aspects common to the group and embody this in the expression of correlation. The deviation of the individual patterns from the ideal norm is expressed by ϵ. Thus the correlation itself and the precision of correlation are group aspects as referred to the particular occurrences. For the correlation to express an experimental finding, and not a mere selection of data, the region of correlation defining the group must be specified in terms independent of the correlation. This may, and often will, be expressed in variables other than those used in the correlation. But the region may depend upon the variables $x_1 \cdots x_n$, or may even be expressed as limited to certain values of p. But in such case p or x must be defined by the process of measurement over the regions considered outside the correlation.

But we are not content merely to tabulate past experience. It is our expectation and intention that our correlation shall describe any future occurrence assignable to the same group, i.e. falling in the region of correlation. We thus shift from the group aspect of a class of occurrences to the pattern of a particular occurrence. The deduction of this pattern is a factual proposition. The correlation previously established is the major premise; the identification of the occurrence as falling in the region of correlation is the minor premise; the deduced pattern in the present circumstances (including initial or boundary conditions) is the conclusion wherein ϵ becomes an estimate of the probable deviation of the occurrence from the prediction.

THE STRUCTURE OF KNOWLEDGE

In the analysis of the function f_j, we have shown how it may progress in an orderly fashion until the precision of our correlation is limited by a fundamental uncertainty, ϵ (n), which depends upon the mode of our description in terms of the n parameters $x_1 \cdots x_n$. In particular we may encounter a fine structure in our phenomena which requires an entirely different mode of description, usually in terms of an entirely new group of variables. Thus the first mode of description will be given by the expression:

$$|p(x_1 \cdots x_\infty) - f_j(x_1 \cdots x_n)| < \epsilon\,(n)$$

The next approximation will be given by the relation:

$$|p(z_1 \cdots z_\infty) - f_k(x_1 \cdots z_m)| < \epsilon\,(m)$$

But the second expression must contain, together with the fine structure, the identical relations comprised in the first. Further, if we may be satisfied with a degree of precision within the limit ϵ (n), the two relations are entirely equivalent. In fact Occam's principle tells us the first expression is preferable since the independent parameters are fewer.

We thus have two alternate modes of analysis of the same phenomena and we may seek the relation between them. We note first that, in the transformation from the x's to the z's, some of the x's may be identical with some of the z's, but that, in the main, the x's may be represented as functions of the z's. We recall that the p's and x's were all metrical properties of the world and that our choice as to which we should call properties and which parameters was entirely arbitrary. Thus, all the x's, except those which are identical with particular z's, are functions analogous to $p\,(z_1 \cdots z_\infty)$.

Thus, letting $f_k = \phi_o$, we get a symmetrical set of relations:

$$|p(z_1 \cdots z_\infty) - \phi_0(z_1 \cdots z_m)| < \gamma_0\,(m)$$

$$|x_1(z_1 \cdots z_\infty) - \phi_1(z_1 \cdots z_m)| < \gamma_1\,(m)$$

$$\begin{array}{cccccc} * & * & & * & * & * \end{array}$$

$$|x_n(z_1 \cdots z_\infty) - \phi_n(z_1 \cdots z_m)| < \gamma_n\,(m)$$

But, from the first mode of analysis, we know that, to the limit of precision ϵ (n), the phenomena could be described in not more than n degrees of freedom. This means that to within corresponding limits the functions $\phi_o \cdots \phi_n$ are not independent. It must be possible to eliminate the variables $z_1 \cdots z_m$ in groups from these equations so as to reassemble our original expression:

$$|p - f(x_1 \cdots x_n)| < \epsilon\,(n)$$

The two modes of analysis to the same degree of correlation may be shown by the expressions:

$$|p(x_1 \cdots x_\infty) - f_j(x_1 \cdots x_n)| < \epsilon\,(n)$$

$$|p(z_1 \cdots z_\infty) - f_j[\phi_1(z_1 \cdots z_m) \cdots \phi_n(z_1 \cdots z_m)]| < \epsilon\,(n)$$

The quantities $p\,(x_1 \cdots x_\infty)$ and $p\,(z_1 \cdots z_\infty)$ are the same metrical aspect of the world referred to two different modes of analysis. The degrees of freedom of the phenomenon are not greater than n, while n is less than m. Thus the m z's may enter only in combinations expressed by the n functions $\phi_1 \cdots \phi_n$. Thus $\phi_o \cdots \phi_n$ are statistical functions of the variables $z_1 \cdots z_m$, and $p, x_1 \cdots x_n$ are group properties of the system (or world) as referred to the fine-structure mode of analysis. The empirical relations between the group properties and statistical functions are the set of formulae:

$$|p(z_1 \cdots z_\infty) - \phi_0(z_1 \cdots z_m)| < \gamma_0\,(m)$$

$$|x_1(z_1 \cdots z_\infty) - \phi_1(z_1 \cdots z_m)| < \gamma_1\,(m)$$

$$\begin{array}{ccccc} * & * & * & * & * \end{array}$$

$$|x_n(z_1 \cdots z_\infty) - \phi_n(z_1 \cdots z_m)| < \gamma_n\,(m)$$

The theoretical derivation of the statistical relation in terms of the fine-structure variables will have the form:

$$|\phi_0(z_1 \cdots z_m) - f[\phi_1(z_1 \cdots z_m) \cdots \phi_n(z_1 \cdots z_m)]| < \delta$$

δ may either equal zero or contain the residuals of mathematical approximations.

Now the actual discrepancies, or fluctuations, which we have

assumed to give rise to the fine-structure analysis, may be further expressed:

$$p(z_1 \cdots z_\infty) - f_i[\phi_1(z_1 \cdots z_m) \cdots \phi_n(z_1 \cdots z_m)] = \eta$$
$$|\eta(z_1 \cdots z_\infty) - \psi_k(z_1 \cdots z_m)| < \epsilon\,(m)$$

This is an actual fine-structure property which may not be expressed in terms either of group properties or statistical functions. There will of course be other properties which may be correlated only in terms of the fine-structure variables.

The significant fact is the coexistence of two modes of analysis in the same region of correlation, one devoted to particular phenomena, the other to group phenomena. Occam's principle indicates that the proper analysis of the group phenomena is in terms of those metrical parameters which correspond to statistical functions of the particular variables. The transformations between the two modes of analysis are the correlations between group characteristics and statistical functions of the particular variables. The group analysis will ignore the particular phenomena, while the significant simplicities of the statistical relations will usually be obscured in the complexity of the particular relations.

We have indicated the emergence of the statistical transformation as a retrospect from the study of fine-structure phenomena. But it may equally well arise from the study of particular phenomena whenever we find correlations with fewer degrees of freedom than the number of selected independent variables. The transformation of variables to a set of lesser number, corresponding more closely to the actual degrees of freedom, will involve new parameters which correspond to statistical functions of the original set. A parallel experience is the common procedure of rearranging the independent conditions of an experiment to a simpler set of specifications which uniquely determine the state of the system studied.

The logical development we have followed is reminiscent of the relation between the kinetic theory of gases and classical thermodynamics. One mode of analysis, the kinetic theory, is

devoted to particular phenomena such as the distribution of velocities, mean free paths, and transport phenomena. The thermodynamic mode of analysis is concerned with temperature, entropy, and equations of state. The statistical mechanics derives temperature and entropy as statistical consequences of molecular motion. But we should note with especial clarity that both particular and group phenomena are metrical aspects of nature.

Hard rigid bodies are elements of experience, and the laws of the lever were scientifically formulated before matter was analysed into atoms. Chemical laws were formulated before the atoms were dissected into electrons and positive nuclei, and the analysis of spectra has progressed without reference to current studies of the transmutation of nuclei. For an understanding of the structure of the world and of knowledge it is essential to realize that there are many levels of empirical observation and correlation. It is often said that nature integrates the equations of motion with a facility transcending that of our mathematicians. What this means for our present discussion is that there are many orders of natural phenomena, each presenting for observation metrical aspects of the world which are related in ways paralleled by our logical processes of analysis and correlation.

The statistical transformation thus appears as a connexion between two modes of analysis. It is a correlation where relations between the fine-structure variables of the first mode give rise to statistical quantities corresponding to the group aspects employed as descriptive elements in the second. But there often arise several different modes of empirical study in the level of group phenomena. These produce correlations in terms of dissimilar sets of group aspects. We always infer that such diverse analyses can be related as statistical correlations in terms of some common, more "fundamental," mode of analysis. In fact, the terms of this "fundamental" analysis may remain a set of symbolic constructs whose only justification is the extension of the region of correlation and the fusion of diverse correlations on the statistical level. These "fundamental" constructs thus symbolize the common, or group, aspects of the diverse correlations.

This is simply a statistical transformation in what appears the inverse direction, but the logic has the identical structure. The "fundamental" constructs are simply the statistical properties of the diverse correlations considered as particular phenomena.

For years this was the status of the atomic theory in chemistry. Atoms and molecules were constructs serving as logical bases for the correlations embodied in the empirical laws of definite and multiple proportions, and the relations between chemical and electrochemical equivalents and the combining volumes of gases. Modern physics, we feel, has given experimental reality to individual atomic phenomena. No one can see the tracks of α rays, β rays, γ rays, and their intersections in nuclear transformations as photographed with the C. T. R. Wilson cloud chamber and escape a personal experience of individual atomic processes. But we should pause a moment to examine what we actually see. We see or photograph droplets of water, macroscopic bodies, from whose arrangement we infer, as a statistical consequence, the passage of the microscopic particle.

To recapitulate: the metrical properties of nature are found in many levels in each of which empirical observation and correlation may be made. By analysis or synthesis of these correlations statistical transformations may be found relating two modes of description. The relations found between particular elements of one mode may give rise to statistical quantities identifiable with the descriptive elements of the second mode of analysis. But there are symmetrical transformations leading in both directions; the relation is reciprocal. We have no a priori criteria as to which aspects are statistical and which are "fundamental."

Much of knowledge is the compilation of direct empirical correlations. But the more elegant and more powerful parts of science are those logical structures expressed in terms of those transformations which we have called "statistical." The famous conservation principles of classical physics and the variational principles of mechanics are mathematical formulations of this type of transformation. The dictum of Clausius, "Die Energie der Welt ist constant. Die Entropie der Welt strebt einem Maximum zu." expresses two correlations on the statistical level

with respect to which all transformations of heat and work are particular phenomena.

On the other hand most of our "explanations" and "understandings" are similar qualitative formulations. A beautiful example is a flame like that of an oxy-acetylene torch. By one mode of analysis it is a stream of interdiffusing reacting gases. Its shape is the envelope of certain stream lines. By the other mode it is an enduring form which may cut a sheet of steel. We may determine the distribution of color and temperature and the flow of heat available in each part of the flame or, as the welder does on the basis of experience, we may predict the rate of cut from the size of the flame and the height of the inner cone and estimate the stability of the flame from the pitch of the radiated sound.

It is doubtful that we have yet found the ultimate building stones of the universe and, if we had, it would be futile to attempt to organize them into the composite statistical concepts requisite for many spheres of human activity. Though we recognize the alternate modes of empirical analysis, the statistical transformations are unknown, would involve too much mathematical analysis or would require a ridiculous degree of accuracy in the determination of fundamental constants. A stream of electrons may be a succession of particles or a train of waves; some constants of the transformation are known but, for most of us at least, its exact nature is shrouded in the mysteries of quantum mechanics. Again, we may examine bacteria under the microscope, compare their forms and reactions to stains or study the clinical aspects of the diseases they produce. But ultimate chemical analysis will give us no knowledge relevant to their significant metrical aspects which we infer to be features of organization or structure.

It is an essential truth that our symbols and transformations find their prototypes in the structure of nature. All the metrical properties of the world as presented to our senses we infer to be features of organization as referred to ultimate constituents. Enduring forms are the basis of geography and are endlessly repeated in the structure of crystals. Allotropic modifications are alternate modes of organization which usually occur in different

regions but may coexist in one experience as diverse statistical expressions of the atomic fields of force. The metamorphosis of a caterpillar is a never varying transformation between two modes of organization on the same observational level. We infer that it is genetically determined in that natural pattern of continuity known as the ontology of the species. In the evolution of species, phylogenesis is an extension of structure and function to a new region of adaptation.

It seems no accident that natural organization should be analogous to the structure of our symbolic representation. It would rather be strange if the anticipatory neural responses of the scientific thinker should not symbolize in structure the world he studies and if their evolutionary development should not follow the universal pattern. Our symbolic activities both represent the world in experience and are an aspect of the world itself; they are essential factors of universal history. And, like scientific knowledge, every art form has its aspect of universal truth, its symbolic structure and its place among natural phenomena. The tone of each musical instrument has its peculiar harmonic analysis but the timbre of each note is a particular element in the harmonic structure of a symphony. And the music itself, particularly pure music, is a symbolic expression of universal or group aspects of human emotion and intellect divorced from the particular elements of experience. Scientific thought is that sector of the search for truth and harmony where knowledge is constructed by the analysis and correlation of natural phenomena and the synthesis of new concepts to symbolize those phenomena in experience. But science finds its entelechy in the purposeful direction of nature through understanding.

CONCLUDING REMARKS

In the foregoing pages we have set up a symbolic expression for scientific knowledge and the scientific method applicable to systems of many degrees of freedom:

$$|p(x_1 \cdots x_\infty) - f_j(x_1 \cdots x_n)| < \epsilon\,(n)$$

It embodies the principles of correlation and the relations between phenomena and those symbolic ideas which represent them in experience. From its structure we have deduced procedures applicable in scientific investigation which should concur with effective methods in current use. A particular field of knowledge will comprise correlations which are each a special case of the generalized symbolic expression. The application of knowledge to a particular instance will involve a factual proposition whose major premise is an established correlation of this type.

The metrical aspects of the world appear in many levels for empirical observation and correlation but a definite structure of knowledge is found in certain correlations which are reciprocal transformations relating the different levels of observation. To a great extent this structure of knowledge parallels the structure of the world whose aspects we observe and measure.

These formulations summarize, for the author, some years' experience, mainly of research in the fields of chemistry and physics. Their truth and value will depend (apart from internal coherence) upon their applicability to the detailed problems of the reader. They should be an integration of practical principles with a close correlation to effective methods in practice.

So our general principle, expressed as one of its own special cases, should agree with actuality within some quantity ϵ which may vary from field to field and perhaps be smaller in the sphere of experience from which it was formulated. But, as an expression of dynamic scientific thought, it should be subject to further extension and refinement, though in ways which are not now apparent; for in a dynamic science there are no static principles but an expanding cycle of understanding approaching, but missing by a quantity ϵ, an ultimate reality.

So we escape the curse of infallibility and, though f may approach asymptotically some particular doctrine of mathematical physics, we never achieve that mad and empty world of meaningless relations where the symbols usurp the place of reality. Instead, each new idea, never leading to a finite conclusion, adds to the breadth or increases the depth of an ever expanding life of scientific thought.

ACKNOWLEDGMENT

In a discussion of scientific method little can be added that is essentially new; we have tried rather to integrate many aspects of human thought. This is not an historical account and acknowledgment is superfluous to those who have founded the scientific tradition. But, in the development of the present article, the author is indebted to Dr. W. S. McCulloch for discussions and criticisms which were its chief stimulus, and to Dr. E. vonDomarus for some ideas which are mainly to be found in his thesis on "The Logical Structure of Mind."[3]

New York, May 5, 1938.

[3] E. vonDomarus, "The Logical Structure of Mind," a thesis presented as part requirement for the degree of Doctor of Philosophy at Yale University, 1930, rewritten 1934.

A Revolution in Metaphysics and in Science[1]

BY

JOHN ELOF BOODIN

TO RECOGNIZE the reality of the space-time structure of nature would, I think, revolutionize metaphysics. It means that we must conceive reality as history and history as having direction. It is not enough to take time seriously, we must take the order of time seriously. When we view the apparent confusion of nearby events, we may indeed be tempted to say with Heraclitus: "Time is a child playing drafts. The kingly power is a child's." But if we take a long view we may discern order. The early Greek pioneers started philosophy on the right track. Anaximander, the real founder of Western philosophy and science, had the brilliant intuition of an order of time as regulating the cosmic process. In the one direct quotation from Anaximander we are told: "And into that from which things take their rise they pass away once more, as is meet; for they make reparation and satisfaction for their injustice according to the ordering of time."[2] It is because of the "ordering of time" that the universe is a self-maintaining whole. The development of the conception of the ordering of time was left to the great metaphysical genius of Heraclitus, who conceived a thought

[1] Paper read before the Pacific Division of the Am. Phil. Asso., Dec. 29, 1937.

See in this connection the author's paper, "Cosmological Implications of Normative Structure," read before the Ninth International Congress of Philosophy, Paris, 1937, and published in its Proceedings.

[2] John Burnet's translation.

running through all things and steering through all things. There is a controlling direction of change which regulates the rhythm of exchange in nature and keeps the universe from running down. It was this emphasis upon the Word, the *logos*, the controlling order of change, which Heraclitus felt to be his important contribution. Change, continuous flux, is real, but there is a guiding order in change.

Had philosophy and science proceeded from the start given by Anaximander and Heraclitus, Western thought would have been a different drama. But after Heraclitus came Parmenides. Parmenides had the profound insight that, in order to have thought or discourse of nature, there must be constancy in nature. But he demanded absolute constancy and ruled out all process as illusion. He failed to see that constancy can have meaning only in a world of change. Science has indeed been in search of constants, but they are relative constants. Atoms are constants for chemistry. But that does not mean for us that atoms are eternal. They are only constants for certain purposes. Parmenides is the great tragedy in Western thought. After Parmenides the emphasis has been on the eternal in the sense of the timeless. The pluralists followed Parmenides in denying intrinsic change. There is only mixture and separation of eternal entities. Plato recognized the existence of becoming, but he accords true reality, *being*, only to the timeless forms. Aristotle, in spite of his talk of movement, regards the moving form (in the last analysis, God) as timeless. Movement implies imperfection—a falling short of "actuality." There is no motion in God. Modern idealists follow Aristotle in regarding time as a mark of imperfection and absent in the absolute, without troubling themselves why there should be imperfection. Modern mechanical science has treated change as an external relation, the shuffling of certain eternal entities, whether the atoms of the old chemistry or the genes of classical genetics. When real change has obtruded itself too obviously to be denied, it, too, has been explained as accidental. Process has been explained through a space ensemble of entities or characters.

In the twentieth century there has been an effort to place time on a par with space in the conception of space-time. But space has first been reduced to an ensemble of points and time to a succession of instants. Space-time has been conceived as a four-dimensional manifold which requires four coordinates or numbers for expressing relations within it. To locate a moving airplane requires four numbers, three numbers for space and one for time. But whatever the convenience of such an artificial reduction of time to a number, real time has evaporated. The ghost of Parmenides has dominated philosophy and science up to the present time. It seems, however, that quantum physics in its attempt to get into closer grip with nature is disclosing the metaphysical reality of time-order. This seems to be the meaning of the famous formula, $pq - qp = ih/2\pi$. A new fact emerges from the order of operations.

But for a return to the intuition by Anaximander and Heraclitus of an "ordering of time," we must go to H. Weyl's conception of cosmic space-time curvature. Weyl finds that it is only by a conception of a cosmic space-time structure that we can explain the constant charge of the electron and the repetition of the space-time structure of the atoms throughout the cosmos. Such facts can be understood only by cosmic adjustment. Weyl has expressed this cosmic curvature in a mathematical formula, but its significance goes deeper than any mathematical expression.

In general the conception of space-time-energy structure means that the future is a determiner of the present and the present is a determiner of the past, instead of the old conception that the past determines the present and the present determines the future. Instead of thinking of nature as having a merely spatial structure—an attitude inherited from Parmenides—we must think of nature as having also a time structure. Nature does not consist merely of a simultaneous order of events but also of a successive order of events and the "ordering of time," to use the language of Anaximander, guides the succession. There are indeed contingencies since the ordering includes the conditions of nature and these have their indeterminate character. They are not merely

functions of the structure. But, on the other hand, there can be no deveopment except in so far as the factors and conditions are guided by the space-time structure.

That the future is a determiner of the present and the past can be illustrated in embryology. We can understand the earlier stages in the embryological process only by reference to the future, to the time-whole in which the various stages figure as moments. The future is a determinant in the preceding stages. The past and present count but they gain their meaning from their relation to the space-time structure of the whole. While all the stages in the process are real, the later stages reveal more of the meaning of the process than the earlier. A life is a time-form which is creatively revealed as the form of the whole history is realized.

That the future guides the present can be illustrated by psychological activity. It is what we are going to say which guides the selection of materials—memories, perceptions, expressive movements. As the meaning emerges in the expression we find that it takes on a character of its own because of the whole-form which guides the expression. In the creative dialectic of genuine expression a new wholeness emerges which is not a mere repetition of the past. We may have supposed that we knew what we were going to say, but may find that the emergent meaning is something different from what could have been foreseen from the inarticulate beginnings. The meaning-in-expression reveals a character of its own. The more creative is the process, the more striking is the novelty. Suppose we are creating a poem or a symphony. A new penumbral insight has emerged with its urge for expression. There is a feeling for a whole which is in the future. For genius (to use the language of Shakespere) "feels the future in the instant." This feeling guides the expression and in turn takes on new meaning in the expression. The composer experiments, selects and rejects, until the time-form of the whole comes to clear realization. The original manuscript of a great poem or a great symphony reveals that many trial suggestions were rejected before the feeling for the time-form found its adequate expression. On a larger scale we can see this ex-

perimentation in the evolution of life, with various trials and eliminations, as recorded in the rocks on the long time-scale and observed in man's experimentation on a small time-scale, in life's constructive effort to realize its time-form. And the same selectiveness occurs on a cosmic scale in the evolution of worlds. Thus the form of the future, even while it is being concreted in the womb of nature, is an active determinant of that which precedes in the course of nature.

We have been accustomed to emphasize the contingency of the future. And there are contingent factors in the physical realm, in the biological realm, in the psychological realm. In the plurality and complexity of the conditions of nature the form may fail, and often does fail, of adequate expression. But we generally neglect to recognize that if the future may prove tragic, because of the contingences of circumstance, the present and the past may prove abortive. It is a mistake to suppose that the past is fixed. It is just the past which is being transformed. Whether it is damned or redeemed depends upon the drama of the future. It was in view of that fact that the Greeks would not praise a man until he was dead. But the death of the individual by no means decides the outcome of his life. He may finish his life with apparent glory which in turn may become tragedy. He may die on the cross and yet prove victor. The comedy or tragedy of an individual life depends upon a more comprehensive drama than individual human life. It is for us by creative experimentation to try to discover the trend of the future and to realize it in the passing movements of life.

The conception of a cosmic space-time structure has implications far beyond physics. It is no small gain that physics has shown that Plato's intuition that there is measure and number everywhere in nature is now scientific fact. The electron, the atomic number of the atoms, emerge under cosmic guidance. We are entitled, I think, to project this principle of cosmic guidance into the whole life of the cosmos. If every electron and every atom has a space-time field stretching into the vastness of cosmic history, we may also believe that life and mind have a space-time structural field as comprehensive as reality. The universe

becomes drama in which the various acts are guided by the space-time structure of the whole, with interaction of various histories within the genius of the whole, with comedies and tragedies according to the "ordering of time." Within this drama there is the Word, the *logos*, the space-time form, guiding the parts towards development. Though there is much failure, the path of survival of the multitudinous experiments is determined by the guidance of the time-whole of the future. The early Greek thinkers thought of a cosmos consisting of multiple worlds. That there are multiple cosmic histories is evidenced by science. The photographs of gallaxies, as shown by Hubble, can be arranged in a serial order from the oval undifferentiated forms to those having the spiral complexity, or even greater, of our own gallaxy. Gallaxies are born and die in the great cosmic drama. Their life is controlled within the space-time structure of the whole, enormous though their time-spans are.

In only one small part of cosmic history do we know much of the cosmic drama. If some cosmic spectator had seen the earth in its earliest stage, he might have pronounced it "without form and void." But a spectator whose life span would enable him to watch the genesis and death of worlds, as the embryologist watches the life span of individual organic histories, could reason from the analogies of experience (as the embryologist argues when from the one cell stage he predicts, in the conditions of nature, a human being) that the bit of cosmic dust which was the earth at the beginning had the probability of a drama whose possibilities we still know only in part. He would not make the mistake of regarding the initial stage as *the* reality, any more than the embryologist regards the one-cell stage of a human being as *the* reality. He would see the early stage as an act in the space-time form of the earth's drama and from his extensive survey would be enabled to see more of the probabilities of the future than we can see, as the embryologist can see the early stages of the embryo as probable moments of an emergent whole of life. While we can know little of the space-time structure of the cosmic whole, our creative reason, ephemeral though our existence is, can to some extent—because it is an expression of the space-time structure

of the whole—divine the laws of the cosmos. There is a community, somehow, of our mind with the structure of the cosmos, as shown in our degree of success in deciphering its laws.

The idealists will probably object especially to the including of space in the conception of reality, as I do in the conception of cosmic space-time structure. They think of space as being infinitely divisible and, therefore, inconsistent with wholeness. But extension does not necessarily mean divisibility unless we make extension divisible by definition as the mathematicians do. For Plotinus soul pervades the body and pervades the cosmos, but soul is indivisible. For Spinoza the attribute of extension is indivisible because it is an attribute of God and God is indivisible. The new neurological evidence indicates that we must think of mental processes in field terms. Considerable portions of the brain may be destroyed without permanently affecting intelligence. The field conception applies to mind as well as to matter. We should speak of *mens extensa* as well as *res extensa*. The field conception is useful not only in explaining the operation of the brain mind, it also helps to make social communication intelligible. Now we must think in terms of space-time fields with their structure and interrelations, whether speaking of material or mental energy, and by material I mean that which has the characteristics of matter and by mental that which has the characteristics of mind.

We are not here concerned with space in the abstract or with time in the abstract, but with space and time as aspects of process. And we hold that this process has structure. If space and time are real and if structure is real, then nature is teleological, not necessarily in the subjective sense, though there are instances of such teleology, but in an objective sense, namely that nature must be conceived as an order of emergent events which have reference to the future and are guided by the form of the future. The spatial order, with its simultaneous events and their linkages, must be seen as a cross section of the temporal order of history. If we take space-time structure seriously, then we must conceive reality as history and we must conceive history as having direction. We must not forget, however, that

the concrete space-time structure includes the conditions of nature, with their pluralism and contingency, and that, therefore, the cosmic process shows failure, as well as realization. Only those transformations are viable which are in line with the direction of history.

To grasp the significance of cosmic space-time structure would revolutionize metaphysics. The old cleavages of matter and mind, upon which philosophers have erected their systems, would become relative aspects. They are not absolute distinctions in nature but find their explanation in the space-time structure of history. The metaphysical materialists or physicalists tell us that reality can be reduced to the level that is investigated by the physical sciences. They look to the earliest stages of a cosmic history for the key to reality. Everything is supposed to emerge from these. The metaphysical idealists have selected a consummatory level of human history, the level of mind, and have treated everything that cannot be expressed in terms of this level as phenomenal. Both types of metaphysics neglect the order of time. They are based upon sections of history which are selected because of the bias of the interpreter. Neither can give a coherent account of reality. To say that reality is either physical or mental means that the philosopher chooses the aspects which fit into his theory and neglects the others. But nature is not divided with a hatchet (to use the language of Anaxagoras). The early stages in embryological history are not more real than the later nor the later than the earlier. Earlier or later stages owe their meaning to being moments in the space-time structure of the particular history. The same is true of earth history. There never was a stage when the earth was mere matter. The earliest stage was prospectively part of a history which issues in life and mind. That is why we find that we need biological categories in order to deal adequately with matter, as we have long recognized that we require physical categories to deal with life and mind. If we view nature historically we cannot make absolute demarcations. We must view it with reference to the future as well as with reference to the past.

Metaphysical materialism and metaphysical idealism have both neglected history. The aspects which each has emphasized are real, but they find their meaning in the process as a whole. The metaphysical idealists have been right that the later stages reveal more of the meaning than the earlier. But the later require the earlier, as well the earlier the later. It is in the space-time structure of the whole process that the selected stages get their meaning. A great deal of our earth, it is true, does not manifest life or mind, but it is, nevertheless, part of the environment of life, and life cannot be separated from its environment. If we hold that the cosmos has space-time structure and that within this structure various histories are interlaced into one world structure, we shall no longer make the mistake of identifying reality exclusively with the section of history upon which we may chance to fasten our attention. There is no stuff but energy and energy exists only in space-time systems in which the future is a determinant of the present and past. Those that have attempted to reduce reality to the past and to the simplest past, that of matter, are proved to be onesided, because, although the past is real, its reality is bound up with the future. Those that have emphasized the consummatory stage of mind are likewise proved to be onesided, because, although mind is real, it is bound up with the conditioning organic and physical stages which give it body. The real substance, the true meaning of nature, is not mind or matter but the space-time structure of history to which mind and matter are adjectival.

University of California at Los Angeles.

Symbolism in Science and Philosophy

BY

WILBUR M. URBAN

IS METAPHYSICS possible? From the beginning of our present sophisticated era we have continually been compelled to ask this question. For myself I confess to a growing impatience with this and like questions. I hold with Kant that metaphysics is a *Naturanlage*, that should all science pass away and should we revert to barbarism, there would still be metaphysics. I am also disposed to think that should mankind develop in the opposite direction, should we become the supermen or the angels which the perfectionists of all times have envisaged, metaphysics would not only remain, but metaphysical insight would be the very life of that perfection. As Dante said, "metaphysics is the bread of angels."

Still less am I impressed with a recent fashionable way of asking the same question, namely, are metaphysical propositions meaningful? This is just as silly a question as the first. Even the logical positivist, when he asserts that metaphysical assertions are meaningless, and proposes to show that they are so by logical analysis, is careful, as in the case of Carnap, to say that they are not meaningless in *every* sense, but only in some special sense which he believes he is justified in making normative. Even when he calls metaphysics nonsense, he is careful, with Wittgenstein, to call it "important nonsense." It is true, of course, that "a little nonsense now and then is relished by the best of

men," but this is the first time, to my knowledge at least, that in the long story of human vagaries and absurdities, the words nonsense and important have been brought together. Surely this is the last stage in sophistication!

WHAT IS METAPHYSICS?

What then is this thing, metaphysics, of which we allow our selves to say such extraordinary things? In attempting to answer this question it seems to me to be all important that we should start with it in its aspect of *Naturanlage*—that we should recognize that, like the prose of the famous play, everybody is talking metaphysics without knowing it.

In Bernard Shaw's *Man and Superman*, Don Juan is trying to get over to the man and the woman and to the perverse Devil himself, his understanding of life and of the universe in which that life is lived. "Life," he cries, "was driving at brains, at its darling object and organ by which it can attain, not only self-consciousness but self-understanding." "This is metaphysics, Juan," retorts the Senior Commander, and he was right in his retort. Juan is here talking metaphysics, as we all are whenever we use such terms as "life" the "world" the "universe," and whenever we attribute to them qualities or activities or ascribe to them ultimate origins or ends. In all such talk we are trenching on the metaphysical and are using metaphysical language.

The first character of metaphysics and metaphysical language is, then, that it makes statements about met-empirical objects—such as life, the world, the cosmos—things which themselves are not immediately experienced but are in some fashion the necessary coimplicates of experience and of its expression. But there is a second and equally important character of all these objects of metaphysical discourse. They are not only met-empirical, in the sense defined, but they are always of the nature of "wholes"—either partial or total. It is precisely when we talk about wholes of any kind, whether in science or philosophy, that we trench on the metaphysical.

Metaphysics is then, the name we give to all those processes of thought by which we seek to round out our experience. We may

also say, with Kemp-Smith that, "it is legitimate, because only as metaphysically oriented is the human type of consciousness possible at all. Only by transcending the immediate can we apprehend the immediate." For our present purposes, I should prefer to put it this way. Only by talking about metaphysical objects can we talk intelligently about the physical. As well ask a man to whistle and smile at the same time, as to talk meaningfully about the immediate without transcending it. It is the meaningfulness of this discourse that the logical positivist denies, for it contains propositions that are not verifiable empirically. Kant, of course, never denied its meaningfulness. He held, indeed, that certain metaphysical propositions about the world or cosmos, the soul, and God are empirically unverifiable. But Kant was too good a philosopher to call them meaningless. For he knew that they constituted in many ways the most significant parts of human discourse, and a large part of his effort consisted in showing just wherein the significance and truth consisted.

SCIENCE AND METAPHYSICS: IF SCIENCE IS POSSIBLE, METAPHYSICS IS POSSIBLE

The significance of Kant's question—is metaphysics possible?—lay in the fact that he assumed the reality or truth of science. On the basis of that assumption he had a norm for the testing of the possibility of metaphysics.

The situation is, mutatis mutandis, the same today. Underlying all the arguments of the anti-metaphysicians is the assumption that we have knowledge—in the sense of science—and this in turn becomes the norm or criterion by which the claims of any other type of supposed knowledge are tested. The important consideration here is the change in our conception of science since Kant's day; what we may, perhaps, call the new scientific epistemology.

In the first place, the conception of science has changed in the sense that, as it is frequently said, science itself has become metaphysical. In the second place, we have a change in our conception of science and scientific knowledge, since the days of Newton and Kant, in the fact that a large part of science is recog-

nized by the scientist himself as symbolic. It is true that Kant specifically recognized the element of symbolism in science, but in the main he assumed the literal character of scientific concepts and on this assumption was alone able, as Bergson points out, to achieve his chief results in the critique of metaphysics.

I shall take up the metaphysical character of modern science first and pass from this to the conception of symbolism as a scientific principle.

MODERN SCIENCE AS METAPHYSICAL

It is frequently asserted that precisely at the moment when the antimetaphysical tendency in philosophy is being revived, the world of science has become increasingly metaphysical. This is undoubtedly true and it is a significant fact that the most severe critics of positivism are to be found among the most distinguished physicists of the day.

Now I do not mean of course that science has not always been metaphysical. When Newton cried, "physics, beware metaphysics," it was because he did not realize that he himself was talking metaphysics. The universal mechanism of the Newtonian physics was itself a metaphysics. What I do mean, is, that modern science is becoming conscious that it is talking metaphysics and acknowledges it.

Now that a large part of science—and that the most interesting and significant (I had almost said the *only* interesting and significant) consists in metaphysical propositions in the sense defined, becomes clear on the most cursory examination. All the special sciences claim to make propositions about wholes which themselves can never be known in the sense of the empirical criterion of knowledge of the new positivist. This is, of course, particularly true of the historical sciences in the wide sense, the sciences of life and of peoples.

It is said that you can not indict a whole people, but that is what we find ourselves doing over and over again. We find ourselves in peculiar embarrassment whenever we try not to. Be that as it may, we can not talk history without talking about peoples as a whole, cultures and epochs, and, as Troeltsch in his

Historismus has pointed out, it is utterly impossible to have history without discourse about met-empirical wholes. It is a commonplace of present day methodology of history that the condition of historical judgments or propositions is the assumption of a *completed series of events*, although in the nature of the case no such completed series is empirically given. And this is true of natural history no less than of human. No theory of evolution can be constructed which does not contain such propositions. The fundamental problems of biology are all metaphysical.

But there are also *generalizing* sciences (to use Ricket's term), which make propositions about wholes. Their method enables them, they hold, to infer from a part, or a limited number of parts, that which holds for all parts, and therefore, for the whole. Physics, for example, has to do with the limitless, and to that extent inexhaustible, world of bodies. It claims to develop concepts which hold for every part of this world and, therefore, to that extent for the whole.

This is even more evident in that part of physics which is historical, that part which claims to say something meaningful about the running down or building up of the physical universe. The second law of thermodynamics, which, as Bergson has rightly said, is the most metaphysical of all scientific laws, is so precisely because it is a historical proposition and, when applied to the physical cosmos, becomes a statement about a limitless, met-empirical whole. More of this later.

It is of the utmost importance to be quite clear on this point. The individual sciences claim to make meaningful propositions about partial totalities only; metaphysics, however, about the totality of totalities, about reality as a whole. This is, to be sure, a real difference, one that dare not be ignored and which we shall find of significance in the sequel. But in principle the situation is the same in both cases. It is customary to say that metaphysics is impossible because the world whole is endless and cannot be known in its entirety. But this in itself proves nothing, for the same is true in the case of the partial totalities of the sciences, in regard to which no question is ordinarily raised. In principle the partial totalities of the particular sciences about which significant

propositions are made, are as empirically unverifiable as the object of metaphysics. This indeterminate character of the material of science does not make striving towards knowledge of the whole illegitimate nor propositions about the whole meaningless. In science, at least as ordinarily understood, indeterminacy and meaning are perfectly compatible.

My conclusion then is, that if science is possible metaphysics is possible. The objections against the possibility of metaphysics can be brought equally against science unless we take a truncated view of science which conceives it as very much less than it is ordinarily held to be. But I go even further. I should say that science is possible *only* if metaphysics is possible. We have seen that one might just as well ask a man to whistle and smile at the same time as to talk about the things of experience without using notions of wholes, partial or total, which go beyond experience. We may now say the same thing of science. As well ask a physicist to whistle and smile at the same time as to talk intelligibly about physical objects without including notions of physical wholes. The truth of this we shall see even more clearly when we come to the problem of scientific symbolism.

It is true that the positivists seek to escape this conclusion. Thus Carnap writes, "I will call metaphysical all those propositions which claim to represent knowledge about something which is over and beyond all experience. . . . I do not include in metaphysics those theories—sometimes called metaphysical—whose object is to arrange the most general propositions of the various regions of scientific knowledge in a well ordered system; such theories belong actually to the field of empirical science, not of philosophy, however daring they may be."[1] I do not believe that this distinction can be satisfactorily maintained, as I shall presently show. The notion of a system of science is not separable from the notion of a whole of reality. Wherever that notion enters we have trenched on the metaphysical whether we know it or not. Either one has pure empiricism—and with it mere knowledge of parts (a collection), or one has systematic knowledge

[1] *Philosophy and Logical Syntax*, p. 15.

and this always involves propositions about the whole. There is no middle ground, as Kant clearly saw.

SYMBOLISM AS A SCIENTIFIC PRINCIPLE

The question, is metaphysics possible, is significant only on the assumption that science is possible, for it is the latter knowledge that is taken as a criterion. It is my further contention that changes, since the days of Newton and Kant, in our views of the nature of scientific knowledge, have altered in a significant way the problem of metaphysical knowledge. This change is the recognition of the *symbolic* character of scientific knowledge.

I suppose that it would be admitted that, culturally and philosophically speaking, few things are more significant about the science of our age than its own consciousness of its symbolic character. It is true that even Kant was enlightened enough to say that what is ordinarily thought of as examples of the pure understanding is to be reckoned mostly as examples of symbolic knowledge. But in the main, Kant proceeded on the assumptions of the Newtonian science of his day, which thought of scientific concepts as literal rendering of reality. It was this assumption, never really criticised, which enabled him to criticise the symbolisms of metaphysics with such devastating power.

Be that as it may, there is no question as to what has happened in modern science itself. The passage from the copy theory to the symbolic theory of scientific concepts is the outstanding development of the new epistemology in science. This movement began in physical science and has been carried out most fully there, but it has not been without its effect upon the other sciences also.

Over and over again we are told by the physical scientists themselves that between what is called Newtonian science and that of the present there is in this respect a great gap. Sir William Thompson's idea of the understanding of a phenomenon of nature is constantly contrasted with the notion of the scientific concept as developed since Hertz. For the former to understand was to construct a mechanical model which should represent *literally* the happening in all its parts. For the latter to under-

stand involves no such literal copy but a relation that is purely symbolic. This contrast of the symbol with the copy theory is a commonplace of present day scientific epistemology.

The point which I wish to emphasize is that physics now works with what Becher calls impenetrable, that is *intuitively unverifiable*, hypotheses. In a sense, he points out, it has to a degree always worked in this way. Even Newtonian physics made use of the *actio ad distans* which seemed to some of Newton's contemporaries, for instance Huygens, unintelligible. Nevertheless in classical physics theoretical constructions followed natural processes into their geometrically intuited details. The fundamental mechanical concepts of force, inertia, etc., purport to be generalizations and descriptions of concretely intuited phenomena. Since the middle of the 19th century the picture has gradually changed. The physics of the field of the 19th and 20th centuries, from Faraday and Maxwell to the Relativity theory, presents in increasing degree, self authenticating formal systems which are not any longer in their details intuitively interpretable. This reaches its culmination in the completely unintuitable character of the Quantum theory, where even the relatively most concrete intuitive theory of Schroedinger already operates with vibration signs in many-dimensional space.

In any case, *one thing is clear*. However, we may interpret and evaluate the notion, a large part of scientific knowledge *is* symbolic, and with the recognition of that fact must come a change in our conceptions of scientific knowledge which greatly changes the character of the philosophical problem since Kant's day.

THEORIES OF SCIENTIFIC SYMBOLISM

This, then, is what I understand by "symbolism as a scientific principle," namely that, paraphrasing Kant's statement, scientific concepts formerly reckoned as copies or models of reality, are now to be reckoned mostly as symbols and that scientific knowledge is, in large part at least, symbolic knowledge.

There are various theories as to the range and nature of this symbolism. With regard to the nature of scientific symbols we have on the one hand, the *operational*, or as it is sometimes called,

the conventional theory of scientific concepts; on the other hand, what I may describe as relational or partial coincidence theories. A really careful treatment of this problem would involve the recognition of distinctions into which time will not permit me to go. I must be content to risk the dangers of over-simplification for the sake of brevity and clearness.

In the case of the operational theory it is not necessary that the scientific concept or symbol shall be a "picture," shall correspond in any way to the realities referred to. All that is necessary is that we shall be able to operate with it. For the relational theory, to be a symbol the scientific concept need be a picture only in the sense that there is partial coincidence, namely, coincidence between relations in the nexus of the things pictured and relations in that part of appearance, or of our experience, which stands for the objects. Thus from the logical point of view the problem of the notion of a "picture" as in the investigations of Wittgenstein, becomes basal for a theory of scientific knowledge.

Various figures are used to illuminate this notion. We have that famous one of Bertrand Russell—of the musical score and the symphony— or Jeans' figure of the map. Perhaps we may take the latter as our best point of departure.

In his recent Presidential address, Jeans discusses the relation of scientific concepts to the "wholes" of physics. He points out that "it is a reasonable requirement that a man should represent each bit of territory in its proper shape and its relatively proper size, that is, literally. But he points out that this apparently reasonable requirement can be fulfilled only when the map is of a very small part of the globe, as for instance, the County of Surrey. The larger the whole, the more symbolic the representation. It turns out then, that the Newtonian mechanics was like the map of Surrey—good as far as it went, but not adequate for picturing the *wholes* with which physics is concerned. The inconceivably great and the inconceivably small were equally beyond its ken."[2]

We shall presently take up the special problems involved in the

[2] *The New World Picture of Modern Physics*, Nature, September 8, 1934.

knowledge of the "wholes" of physics. For the moment we are concerned merely with the notion of symbolism as a scientific principle. It is, briefly, that the principle of literal representation has only limited application—that the coincidence of relations between the physical concept and its object is only partial and that the representation is merely symbolic.

There are, then, two main theories of the scientific symbol. Fortunately for the purposes of our argument, it is not necessary to decide between them. For my own part, I believe, with the antipositivists, that a purely operational theory cannot be carried out. The significant thing here is that for modern science its knowledge is largely symbolic in one or other of these two senses. What then does this position mean?

It means, as we are told over and over again, by a Jeans and an Eddington, a Weyl and a Planck, that to an incalculable degree our concepts of the physical world are constructions, and the knowledge which they give, symbolic. Some draw the inference, as does for instance Weyl, that "science concedes to idealism that this, its objective world, is not given but only propounded (like a problem to be solved) and *that it can be constructed only by symbols*." For myself, I draw the same idealistic inferences as does Weyl, but that is not necessary for the present argument. All that is necessary is the acceptance of symbolism as a scientific principle, namely, that the objective world can be known only symbolically.

This then is what I understand by symbolism as a scientific principle. It means, not only that scientific concepts are in the main not copies but symbols, but also that a theory of scientific symbolism must be made a part of the methodology and epistemology of science.

This notion is not only important in itself, but also so significant for the rest of my argument that further comment is desirable. It has been given much more detailed and specific expression in two recent papers by H. Margenau, an important study of the epistemological and philosophical problems set by the developments of modern physics.[3] He recognizes symbolism

[3] H. Margenau, *Methodology of Modern Physics*, Philosophy of Science, Vol. II, Nos. 1 and 2 (January and April, 1935).

as a scientific principle in physics and considers the problem of the "ontological status" of its symbolic constructs. He adopts the partial correspondence theory of symbols; the constructs of physics correspond to the perceptual world but this correspondence is not a one to one correspondence. The world of physics has, he holds, retained its connection with the perceptible world but that connection has now been placed on a statistical footing.

He finds it necessary to distinguish three classes of physical constructs or symbols, (a) the sensible, (b) the pseudo-sensible and (c) the abstract and wholly "insensible." "Quantum mechanics actually replaces the electron and any other physical system by abstract constructs of this sort." "If the physicist," he tells us, "were to restrict himself exclusively to the use of sensible and pseudo-sensible constructs his dream of an ultimate system of explanation would never come true." In his section on the ontological status of these constructs he insists (p. 165), as his main point, that "physical constructs cannot be said either to exist or not exist; their ontological status has to be fixed in accordance with a more elaborate analysis of the meaning of existence. In particular, the value of a construct bears absolutely no relation to its mode of existence."

The question of the mode of existence, he further indicates, is not so much a physical as metaphysical question; problems of this type can be solved only by an analysis of the notions of existence and being, and this involves a larger universe of discourse than that which belongs to physics.

SYMBOLISM AND PHYSICAL WHOLES

The symbolic character of physical concepts is then quite generally recognized by modern physicists. On the assumption—which I think would scarcely be questioned—that physics is the basal science and that its methods are normative scientifically—we may say that symbolism as a scientific principle is established. Our problem now is to consider the symbol in relation to physical wholes.

The thesis laid down by Jeans—and to this there would in

principle be no objection on the part of physicists—is that physics includes propositions about wholes, and the greater the whole the more symbolic the representation. That physics (as indeed all the sciences) does make propositions about wholes—and that unless science is truncated, such propositions are part of science, has already been established. We may call them cosmological propositions, propositions about the physical universe or cosmos. All such propositions are preeminently symbolic.

The infinitely little and the infinitely great are equally beyond the ken of literal knowledge. The discoveries of modern physics, we are told, have not only given new content to our existing framework of concepts, but have placed that framework in question and remade it. Even our concepts themselves have been revealed as appropriate to moderate dimensions only. The recognition of this fact constitutes, Reichenbach holds, in *Atom and Cosmos*, the Copernican crisis of our time. This crisis has made it clear, not only that the basic concepts of science must themselve be changed (as in our notions of space and time in the theory of relativity) but that the relations of these concepts to their objects is symbolic.

With many implications of this situation we are not here concerned; we are interested solely in its bearing on our argument. The upshot of all this is that science also speaks about the cosmos, about physical universes. It too seeks to round out its knowledge by propositions about met-empirical wholes, about the infinitely great and the infinitely little, and that such propositions cannot be other than symbolic. One consequence should, however, be emphasized because of its bearing upon our later argument. It concerns the status of these theories of the universe—the cosmic theories of astronomical physics. The symbolic character of such notions as the building up or running down of the universe, or its expanding as a soap bubble should be obvious. They are formed by extending to the physical cosmos as a whole, notions taken from narrower regions of immediate experience. Whether, and in what sense, such theories are empirically verifiable is a question which I shall not consider in this context. It may be

that they are to be classed as scientific "myths," as held by some positivists. Enough that whatever truth they have is symbolic and not literal.

LITERAL AND SYMBOLIC KNOWLEDGE IN SCIENCE

A further question concerning symbolism as a scientific principle must be considered before we pass to the related question of symbolism as a metaphysical principle. It concerns the very difficult question of the relation of symbolic to literal knowledge in science.

All modern scientists seem to agree with Kant that "much of what is ordinarily thought of as examples of pure understanding is to be reckoned as examples of symbolic knowledge." The question is, which parts and how much.

An extreme view is that all our scientific knowledge is symbolic and that the mathematical symbols are normative. Thus Jeans: "Our knowledge of the external world must always consist of numbers and our picture of the universe—the synthesis of our knowledge, must necessarily be mathematical in form. All the concrete details of the picture, the apples, the pears and the bananas, the ether and atoms and electrons, are mere clothing that we ourselves drape over our mathematical symbols—they do not belong to nature, but to the parables by which we seek to make nature comprehensible."

I cannot help feeling that a statement such as this raises serious difficulties, difficulties which I find quite common in writings on scientific epistemology. It seems to imply that all scientific knowledge is symbolic, that of number, and that of the "parables." This cannot really be, for the concept of symbolic knowledge is meaningless except as contrasted with the non-symbolic. It is not necessary for our purposes to determine exactly what elements in science are non-symbolic, but that there are such, in Kant's terms "examples of the understanding, is certain." All that we care to insist upon here is that there must be non-symbolic knowledge—a point of importance when we come to symbolism as a metaphysical principle.

Again in this same statement it seems to be implied that math-

ematical symbols must be ultimate in our knowledge of the universe, or, as Jeans expresses it, the synthesis of our knowledge must necessarily be in mathematical form. Now it seems to me to be of real importance that this statement should be questioned. Our knowledge, even of the external world cannot consist merely in numbers, for numbers of themselves "say nothing." The mathematical formula, as Whitehead insists, must be interpreted or it is meaningless. It is, after all, the parables, the non-mathematical symbols that, even for Jeans are necessary for understanding. Numbers merely manipulate. A science, if it were possible, that was merely a body of mathematical symbolisms, would say nothing. Interpretation involves translation into language and this involves the other type of symbolism.

Cosmological discourse, for instance, can never be purely mathematical. There is a great gap between the mathematics of the law of increasing entropy and the dramatic proposition that the universe is running down. The latter simply cannot be stated without the element of "parable," that is by carrying over, through analogous predication, of an image of the sensible world and applying it to the non-intuitible whole. Be this as it may, whether mathematical language is ultimate for science or not—it cannot be for knowledge as a whole. If there is such a thing as metaphysical knowledge, its language and symbolism must be of another type.

SYMBOLISM AS A METAPHYSICAL PRINCIPLE

My first contention was not only that if science is possible, metaphysics is possible, but rather that science, unless it is caricatured, is possible only if metaphysics is possible. My second contention is that just as scientific knowledge is understandable only when it is recognized as symbolic, so metaphysical knowledge must have the element of the symbolic. In short, I shall discuss symbolism as a metaphysical principle.

Now the metaphysical symbol, in this special sense, must differ, of course, significantly from the scientific symbol. As I pointed out in an earlier section, there is a real difference between the metaphysical elements or propositions in science and those which

constitute metaphysic proper. Both deal with subjects of discourse which are limitless, inexhaustible, and, therefore, indeterminate. Both speak of wholes or worlds which are met-empirical and therefore, not observable entities to which the propositions may be referred. But in the case of science they are always partial wholes; in the case of metaphysics proper it is the whole of reality that is ultimately the subject of discourse. Now it may be that the first or scientific form of discourse is meaningful and that symbolism and symbolic knowledge is here possible. It may be, however, that metaphysical discourse in the second sense is not meaningful and metaphysics as knowledge not possible.

I have already indicated why no such fundamental distinction can be made. But the distinction is insisted upon so constantly and raises issues of such importance that the point must be met before we can proceed further.

It is argued that to be able to predicate anything of the whole in a meaningful way it is required that this whole be envisaged as really not the totality of totalities but itself a thing among other things. More technically expressed, if everything is a totality and this totality is not itself one of the everythings, then we have a totality of everything and the totality and so on, so that there is no totality except as a collection. It is thus maintained that we cannot avoid a fallacy necessarily involved in the notion of a totality.[4] Such fallacy is especially evident when it is expressed in terms of the notion of an all-embracing system. Such a system could not be enlarged for it would already contain all that there is. If it can be enlarged it cannot be all-embracing, cannot be all that it claims to be. The very notion of a system or a totality has therefore been rendered self-contradictory and fallacious by being made all-embracing. Neither can its claim to absolute truth be absolute so long as it is possible to subject it to reinterpretation. An absolute system that is not final is a self-contradiction.[5]

[4] See E. G. Spaulding, *A World of Chance*, p. 283.

[5] Appeal is often made also to the "logical theory of types" to prove the impossibility of metaphysics in the sense of propositions about the whole. The theory of types is,

Arguments of this type may be used either to show that metaphysical propositions are meaningless or that while they are not possible about the whole as a totality, the *omnitudo realitatis*, they are possible about the whole as a collection. So far as the last point is concerned I am not able to see that a distinction can be made. Any whole, even a loose connection, is just as metempirical as a totality. Moreover, to say something about the whole as a collection involves saying something about it in a totalitarian sense. If I say of reality that it is plural, "my speech," despite what William James says, "bewrayeth me," for I am asserting that plurality is a character of reality taken in its totality and that is not an empirical but a metaphysical proposition. This Kant showed once for all—even if many moderns have not seen it.[6]

The issues here involved are more than merely dialectical and should not be minimized. It is true that the moment one begins to talk about totality, the world or universe as we do about finite entities and partial wholes one is already on the brink of the unmeaning. To say that organisms develop or evolve is meaningful, to say that life evolves is still meaningful, although its meaning is more difficult to determine. But what meaning is there to the statement that the whole of reality, or everything, evolves? Now I think it must be admitted that we cannot speak about the whole without incurring just such difficulties, but I do not think they are insuperable. In principle science faces the same difficulties.

In the case of science we escape the unmeaning by frankly recognizing the symbolic nature of our concepts and that the greater the whole, the more symbolic the representation. I do not see why we cannot escape the difficulties here in the same way. Literal propositions about metempirical wholes are in the

---

however, becoming increasingly unsatisfactory, even from the standpoint of logic itself, and it is an open question whether its application to this type of problem is legitimate. In any case it is possible to state the point at issue without appealing to this debatable notion. In fact it was already done by Kant in the *Critique of Pure Reason*.

[6] See my *Intelligible World*, p. 53.

nature of the case impossible in science no less than metaphysics. Kant showed, once and for all, that literal propositions about metaphysical wholes or totalities are impossible—that such literal extension of the categories from the phenomenal to the noumenal could lead only to transcendental illusion. But he did now show that they could not be extended symbolically. In fact, he specifically states in the *Critique of Judgment* that they can be so extended, and in this I fully agree with him.

Let us consider certain typical metaphysical propositions of this type. It is said, for instance, that all is flux or change or as, in Schopenhauer, all is will. In panpsychisms we are told that all is *beseelt* or all is mind. Now I do not say that any or all of these statements are true; nor indeed that they are verifiable (although that is a question by itself and depends on analysis of the entire notion of verification); what I do say—and this is all that I am concerned with here—is that such propositions are meaningful. In propositions of this sort what we do is to take some aspect of experience, such as change, will, the mental, and give it a privileged position in the interpretation of the whole. We find such a statement meaningful, not because we can operate with it, or indeed verify it in the sense of the empirical criterion, but because we seem to "understand" with it. But that is equally true of what I have called the cosmic propositions of science. When we say that the physical universe is running down or building up it is equally impossible to operate with such conceptions or indeed to verify them empirically, for the universe here also is a limitless, and to that extent, metempirical, whole. Again when we employ such metaphysical predicates, we know full well that we are not using such notions as will, mind or what not literally—that we have moulded, stretched or redesigned our concepts in order to make them applicable to "all that is"—and that in so far as they are thus moulded, they are symbols—but this is also true of the scientific concepts. In sum, here again there is in principle no difference between the symbolisms of science and those of metaphysics. Sentences or propositions containing such symbols are meaningful in either case.

THE NATURE OF THE METAPHYSICAL SYMBOL: THE NOTION OF FUNDAMENTAL METAPHOR

Granted then that meaningful statements about the whole are possible, it is still clear that such statements will differ in important ways from statements about partial wholes, as for instance the physical wholes of science. There is a difference in the nature of the subjects of the propositions and this means a difference in the nature of the predicates. This difference we may describe by saying that the distinguishing character of the metaphysical symbol is that it is *Fundamental Metaphor*. This distinction has been well stated by Höffding.[7]

"It is the character of all symbols, whether scientific, poetic or religious, that ideas or images are taken from the narrower and more intuitible regions of experience and used as expressions for more universal and ideal relations, which, because of their pervasiveness and ideality, cannot be directly expressed. The metaphysical symbol shares this general character but differs at one important point. It differs from all others, including the scientific, in that it seeks to throw light on existence in its totality or in its innermost essence, by figures which are taken from a single fact or a single side of existence as it appears in our experience."

The figures or metaphors of metaphysics are taken from single aspects of experience, as indeed all metaphors must be; but by reason of the fact that they seek to throw light on existence in its totality, they must be uniquely determined, and this uniqueness is defined in terms of the notion of "fundamental metaphor."[8] The crucial point here is the notion of fundamental.

All thinking, as Nietzsche rightly saw, is "arbeiten mit den beliebsten Metaphoren." Even science works with metaphors

[7] *Philosophy of Religion*, pp. 207 ff.

[8] A somewhat similar view is developed by Stephen C. Pepper in an article entitled *The Root Metaphor Theory of Metaphysics*, The Journal of Philosophy, Vol. XXXII, No. 14, July 4, 1935. The notion of the origin of the metaphysical concept is roughly the same, but the "symbolism of metaphysics "is not related to that of science and the distinctive character of the metaphysical symbol, as I conceive it, is not made clear.

and cannot indeed work otherwise. But the favorite metaphor here is determined by the purpose of scientific operation. If we use the metaphor of a machine—and that is of course what we do when we have a mechanistic notion of nature—we think of nature so, only because only when we work with such concepts can nature be controlled. If we use the metaphor of will or purpose, it is because only by such metaphors can nature be understood or made intelligible to us. Operation and understanding are by no means the same thing. In metaphysics, however, the metaphor must be fundamental, that is, it must be taken from those ultimate and irreducible aspects of experienced reality without which experience could not be.

It is this that metaphysics has always tried to do, however imperfectly. It has sought to discover those aspects of experience which are all-pervasive and thus applicable to everything—in other words, "cosmic variables." One of the latest metaphysical attempts of this sort is that of Whitehead. His constructions are definitely guided by this notion of fundamental metaphor, and he recognizes, I think, the essentially symbolic character of metaphysical knowledge although, perhaps, not with all its implications. I am glad to be able to quote a passage which seems to me specifically to recognize this fact. "Philosophers," he tells us, "can never hope finally to formulate these metaphysical first principles" (namely the experience categories which he assigns to all the occasions in nature). "Weakness of insight and deficiencies of language stand in the way inexorably. Words and phrases must be stretched toward a generality foreign to their ordinary usage; and however such elements of language be stabilized as technicalities, *they remain metaphors* (italics mine) mutely appealing for an imaginative leap."[9] The stretching of words and phrases towards generality means a moulding of our concepts into symbols. They remain metaphors and our metaphysical knowledge is ultimately symbolic in character.

I am not, it should be understood, either arguing for the Whitehead metaphysics or for his use of symbols. Indeed, I find

[9] Process and Reality, p. 6.

great difficulties in this metaphysics and I feel that the imaginative leap for which his metaphors appeal (more particularly his use of feeling and value) makes too much demand upon us. All I am concerned to point out is that Whitehead's metaphysics is, when examined, an illustration of symbolism as a metaphysical principle.

SYMBOLS NORMATIVE FOR METAPHYSICS

Thus far in our treatment of symbolism as a metaphysical principle we have sought to establish two things (a) In the first place, that while the metaphysical symbol, *eo nomine*, differs in important respects from the symbol of science, there is no crucial difference such as would make the latter possible and the former impossible. In the second place, by reason of the difference between the two, the metaphysical symbol is of the nature of fundamental metaphor as above defined. In this notion of fundamental, however, is included an element which marks off the metaphysical symbol in a significant way from those of science. We shall call this the *axiological character* of the metaphysical symbol.

Metaphysics, we found to be that fundamental activity by which we round out our experience through the postulation of metempirical entities. The legitimacy of this activity we found in the fact that without it knowledge and discourse cannot be made intelligible. When, however, we examine this experience which is to be thus rounded out, we find that, when taken in its totality or concreteness, it always contains *value* as a fundamental aspect. A fundamental metaphor must include this aspect, otherwise it is not fundamental.

In developing this point I shall make use of a well known definition of metaphysics by Rickert. "The task of metaphysics," he tell us, "is the unitary interpretation of the total world in which there is both the real and the valuable. (Die metaphysik macht es sich zur Aufgabe die gesammte Welt, in der es Wirkliches und Werthaftes giebt, einheitlich zu deuten). If then this is the task of metaphysics—and I believe that it is—it follows inevitably that to interpret the world as a whole—a

world that actually contains both the real and the valuable, metaphysical concepts or symbols, in order to be fundamental—and adequate, must be, so to speak, value-charged. In other words axiological symbols must be normative for metaphysics.

That certain types of symbols are fundamental—and therefore normative—for science seems to be unquestioned by many. Thus Jeans, in the address already quoted, seems to maintain that the universe is exhaustively analyzable in terms of pure mathematics, and only in so far as that analysis is carried out have we science. But to say that the concepts of the mathematical physical sciences are those which are most suitable to the purposes of the biologist and psychologist, to say nothing of the historian, seems to be the very reverse of the truth. Organic wholes are a part of nature but the concepts of nature, the symbols by means of which their character as living is described and expressed are certainly not mathematical. However this may be (and for myself I hold that even in science the mathematical symbol is not normative, but that there is everywhere present what Bergson calls a "double symbolism") certainly in metaphysics symbolism of an entirely different type is necessary—precisely for the reason that in the total world to be symbolized the *werthaftes* is a determing part of that world. Metaphysical symbols, to be symbols, must have an *axiological* aspect.

An essential part of the notion of fundamental metaphor is then that our metaphors or symbols must include the value side of experience. This fact may be described in two ways. We may say that the principle of the "*primacy of the axiological*", which, although he contests it, Nicolai Hartmann, recognizes as the characteristic principle of all traditional philosophy, is determinative. Or we may say that the language of metaphysics must inevitably be anthropomorphic and its symbols taken from the human and personal, rather than from the abstract and impersonal side of experience. The latter is the usual way of describing the situation and I have no objection to thus describing it, for I am not as much afraid of anthropomorphism as most people. Indeed I am critical and sophisticated enough to see that science itself is no less anthropomorphic; it merely takes its

metaphors from another side of human experience, as in the case of the motion of mechanism. But I prefer the notion of the primacy of the axiological. The symbols of metaphysics must be axiological for the reason that they cannot be fundamental unless they include the value side of experience. Whitehead has seen this necessity in including feeling and valuation among the cosmic variables, but still more clearly when, in *Nature and Life* he speaks of "that great metaphysical truth that all ultimate reasons must be in terms of aim at value."

This general position has been well expressed by Eddington in his *Science and the Unseen World*. In referring to him in this connection, I wish to commit myself neither to all his scientific positions nor to his particular brand of philosophical idealism, but merely to indicate his sense of the difference between the scientific and the metaphysical symbol. He holds that it is of the very essence of the unseen world, with which the metaphysician deals, that here the conception of personality should dominate. Force, energy, dimensions, he maintains, belong to the world of symbols (and to a limited world at that). It is out of such conceptions that we have built up the external world of physics. "What other conceptions have we?" he asks. "After exhausting physical methods we return to the inmost recesses of consciousness."

Without raising the question whether the conception of personality should be dominant or normative here—that is a problem within metaphysics itself—we can at least say that axiological conceptions must dominate, and that such symbols must be normative for metaphysics. Such symbols are dangerous, as all symbols are, even those of mathematics. But what is it that makes any symbol dangerous, whether scientific or other? Merely that it is taken literally. The literalist in science is as much a nuisance as in religion. The literalist in metaphysics, or the dogmatist in Kant's sense, is the supreme nuisance. This does not, however, affect in the least the question of a privileged position of symbols. In the age-old struggle between Idealism and Naturalism, the principle of the former that interpretation must proceed from above rather than from below, is to my mind final. This is the essence of symbolism as a metaphysical principle.

THE HISTORICAL JUSTIFICATION OF THIS INTERPRETATION OF METAPHYSICS

I have now, I think, made clear what I understand by Symbolism as a Metaphysical Principle and why I think this view of metaphysics must be accepted. It remains now to justify this interpretation by reference to the history of metaphysical thought.

For reasons of space I shall have to assume here two things which I have argued at great length in my *Intelligible World*. (1) That there *is* a *philosophia perennis*, a continuous stream of metaphysical thought with identical objectives and identical presuppositions, continuing up until the most recent times; and (2) that this perennial philosophy is essentially a "*value charged scheme of thought*", and can only so be understood. I may say, however, that the appearance, since then, of such books as Dewey's *Quest for Certainty*, Lovejoy's *The Great Chain of Being*, and Von Rintelen's *Der Wertgedanke in den Europaischen Geistesentwicklung* have more than justified my interpretation. What I am concerned to maintain here is that "symbolism as a metaphysical principle" has been an integral part of that tradition.

It need hardly be said to those at home in this tradition that symbolism was a metaphysical principle throughout the entire movement of Greek and Mediaeval thought. Plato's use of myth and symbol was not the lapse of an inborn poet into his native dialect, but rested rather upon the recognition that while his myths were not precisely true, "something like it must be true." It rested even more fundamentally still upon a philosophy of language, especially developed in the *Seventh Epistle* which amounted essentially to the position that no language is possible which does not contain a symbolic element. That it continued through mediaeval times, theology, in both its positive and negative type, makes clear. Our mental pictures of God are necessarily constructions built up from imperfect and human data. The entire doctrine of the *via eminentie*, and, still more fundamentally, the Aristotelian doctrine of *analogous predication* upon which it was built, proves that. But I think Dean Inge is right when he insists that the same assumptions underlie the conceptions of the *World* in this metaphysics. We may summarize

it all by saying with J. Huizinga, that "symbolism is the music of which the logically expressed propositions are merely the score."

As the principle of symbolism is the key to the understanding of the spirit of traditional philosophy, so it is the denial of this principle which is, I think, the key to the understanding of much of modern philosophy. The entire story of critical philosophy since Hume and Kant is, as Mauthner has said, the progressive *Selbstzersetzung des Metaphorischen*. When literalism invaded religion through Protestantism, it invaded philosophy also. But to this was added another element, namely, the assumption of the literal character of scientific concepts. The entire anti-metaphysical movement of modern times is a reflection of the assumption of the literal truth of the Newtonian world picture. It was this assumption, and this alone, which as Bergson points out, made the negative element in the Kantian critique so powerful. In contrast to science, so conceived, the concepts of metaphysics became, of course, *transcendentaler Schein*. There is, of course, a positive side to Kant's thinking, and even he recognized in the *Critique of Judgment*, symbolism as a metaphysical principle and the possibility of metaphysical and theological knowledge when the principle of symbolism was admitted.

In the main, however, the antimetaphysical tendencies in modern science and philosophy arise out of the supposed literal character of all scientific knowledge. But if science itself is not literal—if it is symbolic in the sense and to the extent described, if, in other words, symbolism has become a scientific principle—must not the entire position with regard to metaphysics be reconsidered? Such at least is my thesis. I am inclined to think that both culturally and philosophically, the notion of science as symbolic is one of the most important aspects of modern science. It has brought the entire problem of symbolic knowledge into the foreground and I am inclined to believe, that when this problem is fully worked out and its implications fully realized, the problem of the relation of science to philosophy will take on a new form.

Yale University.

Nature Common Sense and Science[1]

BY

ELISEO VIVAS

THIS paper attempts to explore some of the implications of the distinction between having or enjoying and knowing, between "experiencing" and science, for these implications have not always been adequately realized by those for whom the distinction is fundamental.

Knowledge is a very ambiguous word, and a number of the persistent difficulties of philosophy spring from the ambiguities which the term covers up. At least one source of confusion, however, can be eliminated by refraining from use of the word in the sense of "knowing how." This does not however impoverish communication. For instance, in the phrase, "Paul knows how to swim," it seems that what we mean is that Paul swims when the occasion arises. If this ability constitutes knowledge, it must be distinguished from knowledge which consists of inferential processes, symbolic systems, and techniques of verification.

When the term is used in the latter sense, however, it is still a source of persistent ambiguity. For the word knowledge may refer to the picture or plan of the structure of determinate events or things. In this case knowledge refers to the formulations themselves, in relation to the state of affairs about which they

[1] This paper was read at the meeting of the Western Division of the American Philosophical Association, at the University of Illinois, April, 14-16, 1938.

are, but considered in abstraction from the psychological conditions through which these formulations become available to the knower. But it may also refer to the psychological conditions through which alone the picture or formulation can be possessed, in abstraction from the formulations themselves. In such cases when a man "knows" traditional psychology asserts that he is in the possession of ideas, concepts, pictures, notions and so forth. These entities, or processes, refer, by means of sharable symbols, to events and things of the world. But these events and things, while they may be distinguished in logic from the subjective conditions which make them manifest to us, cannot in fact be had or be known free from these conditions. This is no reason however, as shall be shown below, for asserting that they cannot exist independently of the psychological processes through which we enjoy them or know them.

The formulations and conclusions which are our knowledge are arrived at through an activity which starts by isolating a section of the world in which we find ourselves, in order to discover within that segment or section certain relatively constant and interrelated features which can be symbolized with an increasing degree of clearness and distinctness. The features thus symbolized consist of relational complexes which are more widely pervasive and have a greater degree of invariance than the unique things and events of nature. It is to be particularly noted that the process is from the start a discriminatory one; it starts first by selecting more or less clearly a field within which the analysis is to be performed, and proceeds by further discrimination, selecting from within the chosen field less apparent but more widely pervasive features.

There are innumerable other important details involved in the process of knowledge. The genuinely invariant features of nature are the least obvious ones, and they are sought for through very indirect means. The point however which needs be emphasized by reiteration is that the whole process is a progressively discriminatory one. Our needs and interests first force on us a field of inquiry—the field of objects in motion, let us say, or that within which the phenomena of life manifests itself, or that

involved in relations of production and exchange among human beings; having vaguely marked out an area we then begin the search for invariant relational complexes within it. Thus, in any discipline we never know the field itself from which we started. We know *about* it. Which is to say that we know only the structural features of nature, the system of relational complexes at which analysis happens at any given time to stop. These systems of invariant relational complexes are discovered within the field from which inquiry started. The term "discovery" is an unfortunate one; it should not be taken here as implying a mirror-theory of mind. However, the invariants which analysis finds are not the sole constituents of the isolate of nature from which the inquiry started. Hence no scientific discipline discovers "the ultimate" or "the essential" or "the real" reality of nature.

The claim that the structural features of nature which any science discovers make up its exclusive or its real reality, involves, it would seem, two errors. The first could be called the Platonic prejudice, for it consists in the belief that the transient or the perishable is less real than the relatively fixed or permanent. The other might be called the analytic fallacy, and this paper could be considered an effort to expose it. Stated in brief, the analytic fallacy consists in assuming that the terms of the relational complexes at which analysis stops at any given time are the sole consituents of the things from which that analysis started. Nineteenth Century materialism committed this fallacy; logical atomism does too. And I do not think that it is an exaggeration to say that in one way or another it can be found steadily vitiating the tradition of European speculative philosophy from its beginning. That it is a fallacy is shown adequately, it would seem, from the fact that with the end-terms of an analytic operation alone we cannot construct again the natural objects of nature from which these end-terms were extracted.

It has never happened in the history of European science that the activity of knowledge in any given field has rested satisfied with any given set of relational complexes; it has always sought to go beyond. Thus it is that the terms of the relations found at

any one time become in turn the starting point of analysis which breaks them up into further relational complexes, and in these relational complexes we will notice again terms which for some time remain unanalyzed "data," with which analysis stops. In the terminology of this paper, they are called "the end-terms of analysis." Thus at one time physicists stopped at relational complexes the terms of which were atoms. They have now gone way beyond the atom. But although current usage justifies the employment of the term "datum" as synonymous with the expression "the end term of an analytic process," its usage in this sense is not a very happy one, for there is nothing so far from "the given" as these end terms. The given should be gratuitous; it should be free for nothing. But these terms of analysis are paid for at the heavy price of severe analytic effort. They are not given to us, we have to go after them. The given proper is the complex and much less discriminated world in which common sense dwells and from which any science whatsoever starts. The epistemologists get their "data" from the psychologists, or get it in their capacity as psychologists. They do not always realize that since it is, as they often admit, an abstraction, the given of which they are talking is only a partial constituent of experience and most emphatically cannot be a fixed or unalterable element, except as the needs which instigate analysis remain content to stop at it and not to push any further.

The nature in which mature common sense lives is one in which there is already a great degree of discrimination; a discrimination performed by us more or less unconsciously in our effort to satisfy our needs. The systematic sciences started from this already more or less discriminated nature; they started with a working fund of recorded invariants which had been formulated with a rough degree of precision and of clarity. And this of course is the reason why any exact science has in its nonage the job of verifying and refining upon the discriminations of common sense. In addition the sciences have had the task of improving the rough tools of analysis with which they started. The statement of course is not meant to do full justice to the complexity and difficulty that had to be overcome. It serves

however to point to the progressive task of science in its barely essential aspects. And it is introduced in order to make clear the following points:

(1) Analysis does not stop at any particular point because nature inherently resists analysis. The notion that nature inherently resists analysis beyond a certain point rests upon the assumption of ultimate simple elements out of which it is compounded. The above considerations and the history of science should make us suspicious of such an opinion. There are no doubt simple elements, but they are such for a particular analytic procedure, for particular purposes. The history of science would seem to show that when the scientist stops at any given point it is because of extraneous practical reasons—because he has no time to go any farther, because his instruments are too rough to enable him to go on, because his curiosity flags, or because the practical needs which so frequently furnish him with the motive-power of analysis have been satisfied with the results already obtained. Whatever the reason, however, it does not furnish us with grounds for assuming that science has at last touched ultimate bottom.

(2) The second point is that nature as it is for common sense at any period for any culture is already a nature whose character is what it is as the result of discriminations which have become habitual and are taken for granted. These past discriminations, the result of scientific activity, are part of the social heritage of the members of a given culture, and are found in solution in the institutionalized and conventionalized patterns of practical living. As they are gradually made, they enter into the common social heritage of the culture in which they are made, and alter by degrees the character of the objective world as it is presented immediately to the members of that culture in their unreflecting experience. Our world is what it is for us today because of what we and our ancestors have done to it and with it in the past. Human curiosity and the efforts to exploit it have transformed it for us, and our "given"—the world as it appears to our unscientific eye—is relative to these factors. The nature in which the lake dwellers of Switzerland lived is not the same as that in

which the Greeks lived, nor is either of these the same as that in which we post-Darwinians live.

(3) If this statement holds, the absolute character of the dichotomy between knowing and having breaks down. And it is necessary to break down the absolute character of this dichotomy, because taken absolutely it makes it difficult if not altogether impossible to explain how knowledge can be effective in nature, and how the character of the world can change for unreflective experience.

The acceptance of these remarks forces us to turn deaf ears on the claims made by some scientists to the effect that their science discloses to them "the nature of reality." Reality can have no single nature, no unique essence, since it has no bottom. As we have seen, to ask what is "the nature of reality" is to give rise to a process which isolates some determinate segment of nature in terms of specific aims, and which leads to the formulation in symbolic terms of the structure and behaviour of the events which occur within the abstracted segment. These systems of symbols representing abstracted aspects of nature refer of course to elements which are truly constitutive of nature. But being the result of an abstracting process, it should be reiterated that they do not by themselves alone make up the nature from which they were extracted. Some things are lost in the process of analysis, some things are disregarded, some things are deliberately thrown away. The skeleton we have left is not therefore the whole body, though of course it is a part of it, and an exclusively important part for the purposes in answer to which it was isolated.

These remarks apply to philosophy as well as to science. Mystical philosophers like Bergson who crave to possess the world are naturally impatient with science, because science does not enable them to dissolve within their own skulls the objects which it undertakes to analyse. Of course science does not. But neither does metaphysics nor any other discipline. The only way in which we could possess a thing is by devouring it; and once we swallow it we lose it. Knowledge, as Santayana has somewhere said, is not devouring. In so far as metaphysics

is a discipline capable of yielding knowledge, all it yields is a symbolic system which points to features of the world which the metaphysician takes to be utterly generic. If the metaphysician chooses to refer to those features of the universe which his analysis discovers as "the real,"—setting aside the fact that these features are frequently discovered through an inadequate empirical method—the designation of exclusively real is quite an arbitrary one, and one which by the same logic the physicist, the physiologist, and the economist, have the right to apply to the end-terms of their own analytic efforts. And if the end-terms of all analytic processes are "the real," it follows as a matter of simple inference that things have as many "real natures" as there are aspects of them which can be selected from them for the purposes of analytic inquiry.

All of this is to say that "reality" being a predicate of all objects of knowledge, is not an exclusive predicate of any of them. "Reality" is really a function of interest. Our attention being selective and exclusive, when curiosity is aroused we claim that it be the real which is most centrally within its focus, and that to be most unreal which is farthest away in the grey penumbra.

These remarks do not obliterate of course that distinction beloved of philosophers between hallucinations and things outside of the mind to which our ideas or images correspond. But the remarks do indicate that the existential status of objects as objective is defined in terms of concrete operations guided by distinct interests. This is the case for the objects of common sense as well as for the objects of science. The existential status of objects is verified in terms of operations. In this respect, as in every other, the difference between common sense knowledge and refined science lies in the different degrees of precision and clarity with which they are satisfied. Common sense is satisfied with an inspection which does not demand the use of precision instruments and careful check up in order to accept the results as adequate. The penny one of us claims to be out there on a distant table (for philosophers, like ordinary mortals, cannot very well get along without their pennies), turns out not to be there if the rest of

us cannot see it or hear its dull ring. Most of the objects of precise science, however, since they are not open to direct inspection, require delicate and complicated operations to verify, and demand competence in techniques which not all men possess either the time or the intelligence to master.

To raise the question regarding the existential status of an object is therefore to give rise to the knowing activity. There is never a question for pure enjoyment of the existential status of its instruments, since for enjoyment there are no *questions*. To raise any question is to give rise to an analytic process, is to go from enjoying into knowing. In other words we only know an object to exist *when we know it*, and then we only *know* it to exist because it enters into the knowledge relation with us. But then it is not an object of direct enjoyment. Objects of enjoyment are said to exist in retrospect. The object now known is the structure, which is a selection from the object from which inquiry started. And we know that objects exist independently of our enjoyment of them through inference, because through knowledge activities we are led to hold, in preference to any other, the theory that objects exist before they are enjoyed, and are enjoyed before they are known. And if we should be asked how we know this, we can only answer that the knowledge activity itself, inquiring into the grounds of our knowledge of the existential status of objects, demands this assumption. This is the answer to solipcism in so far as its claim is a knowledge claim.

A consequence of the above is that at the level of the exact sciences the objects to which we grant existential status are social objects, objects which are verified as existing outside the mind of all the members of the group taken distributively. Hallucinations and illusions and the like are objects of enjoyment for which existential status is claimed and which can not substantiate that claim through the determinate techniques of verification which exist for that purpose. Objects which have existential status are objects of enjoyment which can substantiate their claim when the question is raised. The type of existence we recognize the object as having is determined by the technique which verifies it. But there is no question however of autono-

mous realms of being because objects of knowledge, the concrete end-terms in nature to which scientific formulations refer, are constituents of the objects of enjoyment from which the inquiry started. Knowledge does not go out of nature into another realm. It discriminates within nature.

If these remaks hold, the whole question as to whether the universe is made up of matter or of spiritual stuff is a pseudo-question, born in the sin of error and confusion, and therefore doomed to live in the din of wrangling and conflict. The problem arises when we inquire into the nature of knowledge and carry on our analysis along psychological lines. We then discover that knowledge consists of ideas, concepts, notions, and the like. These ideas, we admit, have a content, but since we are carrying on a psychological analysis, we are prevented by our initial selection and by our procedural techniques from going beyond the ideas and concepts and notions which are the end-terms of our analysis. We then forget that the end-terms of analysis are not the exclusive constituents of the things from which we start, and proceed to claim that the things from which we started are *nothing but*, or are *merely*, the things with which we finished. This can be stated in a slightly different way by noting that the objects of the individual sciences are discovered through the activity of the scientist, and this activity is one of the objects into which the psychologist investigates. But the psychologist has no right for that reason of pronouncing on the nature of all the objects of knowledge. To claim then that all objects of knowledge are mental objects is to forget that the only objects he knows anything about are those of his own science, and there are certainly many more sciences in earth, if not in heaven, than his own.

After all that has been said it is only for the sake of an emphatic summary that it needs to be reiterated that the problem as to "what the world is ultimately made up of" arises when the objects of any one department of inquiry are exclusively claimed as generative of the totality of nature from which the inquiry started by abstraction. We know objects in terms of techniques of inquiry which verify and make clear and exact these objects as

being real but not exclusive constituents of the nature in which we live and act and which we try to know. The nature in which we live and act however has for direct enjoyment the character, organization, and regularity that it has, as a result of specific inquiries which have been conducted about it, and which modify progressively the immediate non-cognitive recognition, or the non-inferential awareness which is ours as we enjoy it. The world of enjoyment, in which we live and act habitually, is then an amalgam so to speak of our present interactions with our environment; our doing to it and our suffering from it; and of what in the past we have found about it. Of course past knowledge is modified by present findings as much as present findings and activities are determined by past knowledge. The distinction between knowing and having is a logical distinction. For living animals there is no pure knowledge free from having—some undefined terms which require further analysis always are there; nor is there any pure having which is not to some degree discriminative and hence involves some knowledge activity. The distinction however is basic for philosophy and science in so far as these disciplines pretend to make deliverances about the ultimate nature of things. The distinction of course does not make philosophy otiose, for philosophy is just the very activity of making distinctions of this sort: It does make metaphysics suspect, however, if by metaphysics be meant the discovery of the generic traits of the world. The truly generic traits of the world are those discovered in physics. What metaphysics discovers, since it can not be verified by any truly objective and precise operations, is mostly verbalism. But above all the distinction here explored deflates scientists when they assume, consciously or unconsciously, the rôle of metaphysicians, and tell us, in terms of their own specialized interests, what is "the real nature of things."

Department of Philosophy, University of Wisconsin, Madison, Wisconsin.

The Verifiability of Facts and Values

BY

RAY LEPLEY

THE pervasive contrast in modern life between certainties of science and technology, on one hand, and the uncertainties of human go and policies, on the other, is the objective sou of a fundamental conflict in theory. It is n widely held that facts are rather definit verifiable, but that values are unverifiable;[1] though other stude assume that values are also verifiable in some significant se and degree.[2]

The situation in theory is evidently in part a matter of word the ambiguity of the terms "fact," "value," and "verificatio Facts are often taken as something objective, values as s jective. To some verification means final, demonstrable pr to others it is a matter of degree in testing or confirmat "Testability" might be a better word to use, but "verificatio "verifiable," or "unverifiable" is the language of the writi cited.

Consideration of the various interwoven issues involved le

[1] E.g. by R. Carnap, *Philosophy and Logical Syntax*, 1935, pp. 24–25; C. Krusé, "V nition and Value Reexamined", *Jour. Phil.*, Vol. XXXIV (1937), pp. 225–234; H. Osb *Foundations of the Philosophy of Value*, 1933, pp. 70–77; Charner M. Perry, "The trary as Basis for Rational Morality", *Intern. Jour. of Ethics*, Vol. XLIII (1933), pp. 141; and B. Russell, *Religion and Science*, 1935, p. 249.

[2] E.g. J. Dewey, *Essays in Experimental Logic*, 1916, p. 386, also *The Quest for tainty*, 1929, Ch. X; J. Mayer, "Comparative Value and Human Behavior", *Philoso Review*, Vol. XLV (1936), pp. 491–92; and N. P. Stallknecht, "The Place of Verifi in Ethical Theory", *Jour. Phil.*, Vol. XXXIV (1937), pp. 150–156.

the present writer to conclude, for reasons very summarily indicated in what follows, that facts and values are verifiable in much the same manner and degree.

Any object (thing, relation, or act) may be viewed and expressed both as an existent—an event as such, and as something of interest to an experiencer. "Water is H_2O" is an example of the first; "Water is good to drink" or "It is good to maintain life by drinking water" illustrates the second. "Facts" in a broad generic sense are formulations regarding the existent or possible *as such*. "Values" in the same sense are any interest in any object or any object of any interest. Paradoxically each may subsume the other: the study of the nature or properties of events as such is a major human value, and having interest or valuing is in experience an all-pervasive and important fact. But the paradox is resolved or mitigated by recognition of the psychological polarity of interest: no attention is given, even to an event as such, without the presence or arousal of interest, and there is no interest without an object, present or sought. In every experience the factual and the valuative are inextricably wed, though often only unconsciously or potentially so.[3]

It will be noted that in both facts and values, as thus conceived, there is a relation between experiencer and experienced.

[3] Though the matter cannot here be considered in detail, this polarity appears to point to the conclusion that every factual formulation can be transposed into valuative terms, and every valuative formulation more or less completely into factual terms. For instance, "Water is H_2O" may be expressed as "For the purpose of accurate chemical denotation, water is correctly symbolized as H_2O", "For certain scientific purposes, H_2O is a good (or the best) statement of what water is." The statement "Water is H_2O" conceals the fact that there is interest in making or using the formulation, whereas the other statements express the interest as well as what is asserted or predicated regarding the nature of the event (water) as such. Likewise, the statement "Water is good to drink" can be expressed more factually as "Water is enjoyed by or satisfies a (or this) thirsty organism", or "Water is a substance which assists in maintaining normal physiological processes." If all formulations are thus capable of expression in both factual and valuative form, it follows that verification of the one is verification of the other, and that all possible facts and values are, therefore, if compared *en masse*, equally verifiable. It should be noted, however, that although an affirmative answer to the more strictly semantic question of the translatability of facts into values and of values into facts will support the conclusion herein presented regarding verification and verifiability, such an answer is not necessary to the present argument.

Hence both subject and object affect both fact and value. T as he may even the scientist cannot escape the egocentric p dicament. Fact is always in some uncertain degree the prod of human purpose or viewpoint. Even an automatic counte limited to counting and to what it is used to count. Nor is va merely a matter of subjective wish or preference, it is alwa affected by the properties of the object of interest. Like fa value is a product of *interaction with* objects, whether the obje be physically or only symbolically existential. Water doubtl is characterized or constituted by such modes of action as mak possible for man to perform operations which rather consisten support the formulations "Water is H_2O" and "Water is good drink." Doubtless water is thus characterized or constitut whether anyone knows these modes or not, whether or not anyo has performed operations which support these or other generali tions. But as far as the facts and values can be known, they known through interactions with water. The properties both the water and of the experiencer affect the relations disclosed created in interaction. To perform operations of inquiry wh disclose whether or in what degree there is correspondence tween a particular formulation of fact or value and the actual possible existence of events or properties is to test or verify.

Modern science has made dramatic the fact that knowledg the most distinctive meaning of the term is a product of exp mental interaction. Not all attractions or aversions which a in solving a factual problem are true, though in a broad gen sense they all are "known." Only those suggestions which st the test of further and repeated mental and more overt op tions are known in the distinctive sense. "Facts" are known truly facts—so far as they can be or are yet known—only thro inquiry and testing, processes which are both overt and n overt, individual and social. But no matter how well es lished facts are, whether they be more specific or more ge classes, they are never, in the moden scientific way of think irrevocably beyond the hypothetical stage. All facts are rela though some may be extremely well verified.

No less dramatic is the lesson of art and esthetic the

Creative art has in all ages been an interaction between the inner and the outer, the social and the individual. But only in modern theory have the intimate interplays of energies in creative experience been clearly envisaged and expressed.[4] It is now apparent that we need hold neither that the beautiful is special essence or the transcendent universal shining through the particular nor that all expressions of emotion or meaning are equally artistic or expressive. Adequate experience and embodiment are experimentally achieved and approved. An act or work of art is seen to be truly artistic or esthetic in the degree that it is expressive both at the moment and for continuing enrichment. The artistic and beautiful (or ugly) are "verified," so far as they may be, through an experimental process which, like science, proceeds by observation, by reflective thinking, and by performing and responding to the effects of more overt as well as non-overt operations.

If a similar conception may properly be applied also to more personal and social ("moral") values, they too will designate critically tested formulations about the worth of interests, including the objects of interest. All values, whether scientific, artistic, or moral, should then be conceived as verified, as far as possible, in the same way: through experimental interaction. In no case should complete, unquestionable verification be regarded as either necessary or possible. All values (including facts) should be regarded as relative so far as human experience discerns.

But is not this merely a superficial evasion of the real difficulties? Is any *genuine* verification possible in moral or esthetic matters? In matters of (scientific) fact it is possible in very large measure to rule out wish, to wish only for the truth, and to let the events speak for themselves. But in case of value, it may be asked, (1) Is there as in our perceptual experience any arousal of expectation which is not subsequently disappointed?[5] (2)

[4] E.g. by E. F. Carritt, *The Theory of Beauty*, fourth edition, pp. 215–217, 287–295; J. Dewey, *Art as Experience*, Chs. IV, V, and pp. 167–169; Malvina Hoffman, *Heads and Tales*, Ch. II; D. H. Parker, *Principles of Aesthetics*, Chs. IV, V, VII; and M. Schoen, *Art and Beauty*, Chs. III, IV, V.

[5] Osborne, *op. cit.*

Is not value a matter of inner wish, not of invariant relations discernible through action of the body machine?[6] (3) Is not value deliberation a matter of the creative interplay and satisfaction of wishes rather than their exclusion in order to perceive facts?[7] (4) Are not value statements merely normative, unverifiable imperatives which express human purpose or wish?[8] (5) Is not value judgment primarily a choice of a course of action, a choice which is ultimately arbitrary, having no determinate base either in existential fact or in rationally grounded principles of choice?[9] (6) Are not facts verifiable because they are matters of the space-time continuum whereas values are of a different order of reality?[10]

If we disclaim any pretense at absoluteness in verification or testing, if we adequately recognize our human predicament not only with respect to values but also with respect to facts, and if we also view both "fact-finding" and "valuation" in terms of interactions which are in various degrees experimental, the foregoing questions may be answered[11] briefly as follows:[12]

(1) In matters of value the arousal of expectations which are not disappointed is a matter of everyday and continuing occurrence. When and as needs of hunger, thirst, rest, recreation, disease, et cetera, recur, the objects (things, relations, acts) which have proved useful, satisfying, and healthful are commonly

[6] F. H. Knight, "Social Science and Social Action", *Inter. Jour. of Ethics*, Vol. XLVI (1935), p. 21.

[7] This question is raised and discussed, though not in relation to verifiability, by R. B. Raup, "Limitations of the Scientific Method", *Teachers College Record*, Vol. XXX (1928), pp. 212–26.

[8] Carnap, *op. cit.*, and Russell, *op. cit.*

[9] Charner M. Perry, *op. cit.* and also "The Relation Between Ethics and Political Science", *ibid*, Vol. XLVII (1937), pp. 163–79.

[10] Krusé, *op. cit.*

[11] In replying to these questions, the writer does not presume to have presented fully nor to have refuted *in toto* the positions taken in the writings just cited. The references are given to assist any reader who may wish to examine answers which differ, in the main, from the present viewpoint.

[12] The numbers of the discussion correspond to those of the questions, but due to the fact that the questions are closely interrelated the discussion on any one question is not in every case confined to its respective number. The remarks should be considered collectively.

found to be enduringly valuable, though new ends and means may arise and prove to be of more or less permanent worth. In so far as different persons have common needs and interests their relatively verified values are in agreement at many points, though uniquely different in other respects due to differences in needs, habits, and interests. Some substances, arrangements, and types of action are of value to all peoples, others are of only local or individual worth. There is sufficient constancy in objects and needs to make it possible for man with varying success intelligently to prize and seek some ends and to strive to avert others, possible for expectations to be aroused and dependably fulfilled.

(2) In the determination of value, wishes *are* more largely and directly the subject matter than in the discovery of fact. But to treat valuation as merely subjective, a matter of wish cut off from the conditions which give rise and test to interests, is as unjustifiable as to separate knowing from similar interactions both overt and non-overt, social and individual. Interaction amid social and physical, physiological and psychological conditions and events give rise or direction to desires, and frequently assists in discovering the effects to which various lines of action lead. Effects react to inform, perchance to reform or form desire, to discover ends and means of more enduring worth. Thus man creates and reshapes modes of government, education, child-care, medicine, housing, road-building, communication, friendship, et cetera, which in ultimately indeterminable degrees are found valuable, sometimes increasingly or decreasingly so.

The discovery of new facts and values go hand in hand, and both are total interactions in which judgments have their rise and further test. There is some significant determinateness both in external objects and in more internal habits and needs. Even if choice is ultimately relative to the nature of the self and the self is a more or less chance and indeterminate formation which does not in its inherent nature provide many definite criteria for discrimination, there is some significant determinateness of needs and interests relative to each personality and culture in the degree that it has inherited or acquired distinctive traits.

In the degree that the world is one of perpetual novelty and

that a characteristic human need—when not perverted or crushed—is for further experience, we may not reasonably expect to do more than formulate the general principles of procedure by which we cope with life's situations. No more in personal and social than in scientific or esthetic problems may we hope for ultimately fixed procedures and accomplishments. Changes in conditions give existential base (physical, social, personal) for new facts and values. The experimental method of their continuing creation or discovery is more important than the assurance of uniformly determinate subject matter. Such determinateness as makes the further quest for fact and value possible and in significant degree rewarding is doubtless more important than a simplicity and fixity which might make for complete and constant determinableness.

(3) In personal-social and esthetic adjustments it is necessary to give free play and interplay to wishes as condition to fruitful suggestion and wise choice. But the suggestions which rise here, like those which spring up in scientific problem-solving, are properly subject to careful criticism and more overt testing. This is true even though intrinsic enjoyment has a more important place (for most persons) in personal and esthetic matters than in scientific inquiry. In so far as interests are intrinsic they are goods in themselves. And certainly one end, if not *the* end, for which man strives is to maximize such intrinsic experience. But interests are not only intrinsic; they are also instrumental to other correlative and subsequent personal and social effects, whether trivial or important. The importance of the effects can be appraised in their general tendencies as well as in their immediacy only through the arts of intellectualization, which are assisted by and integrally related with more overt action—past, present, and future.

(4) The same consideration applies also to the view that values are normative or imperative. Unless the normative and imperative are to be vested with supernatural or peculiarly universal authority, wishes and purposes must be regarded as rising within, affording suggestions to, and being (relatively) tested through experience itself. The testing admittedly is often less controlled

and decisive than is *some* scientific inquiry regarding the nature of events as such, but the general *modus operandi* of verification, of genuine inquiry and testing, is basically the same in any case.

(5) Some judgments made in regard to value are concerned with the properties, endurance, and conditions of objects which afford satisfactions as well as with the immediate choice of courses of action. Moreover, the judgments which mediate choice may be divided into descriptive and valuative. Each is essential in any choice, no less essential in scientific matters than in moral or esthetic. Both more factual or descriptive and more normative or valuative judgments are involved in choice of problem, in distinguishing the relevant from the irrelevant, and in interepreting data. But in no case is a judgment, whether factual or valuative, born with certificate guaranteeing its legitimacy or adequacy. Since both facts and values are qualities or products of creative interaction, neither rides always securely and correctly upon the "existential," upon those events to which they refer. Testing through various and devious processes may or may not reveal close and continuous relationship between statement (of or about fact or value) and the actual or possible. But only as attractions and aversions are, by conscious or unconscious choice, carried into further action (overt and/or non-overt) is there possibility for fact and value in the distinctive sense.

In those personal and social situations which offer alternatives which are equally worthy of desire, the choice is "moral" only in the discovery of this fact and in making an arbitrary choice if the making facilitates adjustment or advance. The choice among ends and means of course may be arbitrary, i.e., relative to individual or cultural interests, often without significant basis even in universal social, biological, or psychological needs. Yet in so far as principles do rise as generalizations within and function for aid of overt and non-overt adjustment, they need have no more "ultimate" rational basis or justification. They too may be ultimately hypothetical or "experimental" in character. They rise within and receive test by experience itself. In personal and social action as in scientific programs, interaction may (relatively) inform and reform the arbitrariness of desires

and principles. Whether values as thus relatively verified are ultimately relative to human interests is no more certain (nor uncertain), if we are consistently experimental in our outlook, than that the laws and other facts of the sciences are but temporary averages or individual histories.

(6) Doubtless some formulations of fact are highly verifiable because the events or relations to which they point are for most part unchanging, with rather fixed spacial-temporal properties. But the value of the events may also be enhanced thereby. To cut value off from the spacial-temporal, which certainly includes physical, economic, biological, and at least some psychological events, and to treat it as based upon or resident in a special mode or realm of reality is to neglect the determinate affect of familiar objects in the case of values and of human subjects in the case of facts. Adequately conceived, both facts and values are modes or products of interaction; it is probable that neither would be wholly determinate nor determinable even if the events to which they refer were entirely static and unchanging.

The viewpoints thus briefly discussed seem to suffer from a common defect. They fragmentize the actual and potential continuities of non-overt and overt interaction. Wish, choice, judgment, and knowledge tend, in matters of (moral) value, to be cut off from the conditions which give them rise and further effects—effects which in varying degrees can be administered and utilized for knowing, whether in science, art, or social and personal affairs. The precarious status of fact is overlooked or minimized, that of value is maximized.

The root source of difficulty and confusion in our attempt to conceive adequately or correctly the relative verifiability of facts and values is perhaps the tendency to take as typical of facts the most determinable and as typical of values the least determinable. We do this because science has dealt with the relatively simple, constant, and controllable, hoping thus to be able in time to unravel the more complex and changing. The successes of science have made dramatic the more easily verifiable statements about relatively simple and isolated events or relations. On the other hand life forces us to deal with the more complex and in-

determinable facts and values in personal and social affairs. In this range of experience, therefore, the uncertainty of our formulations is made vivid. The certainties of scientific facts are set off in sharp contrast to the uncertainties of human goals and policies.

It is, then, of paramount importance both for theory and practical action to recognize that if we conceive the present conditions as being the result of fixed, inherent differences between facts and values, between science and technology as dealing with the determinate and human goals and purposes as dealing with the indeterminate, we may be basing our conclusions upon data which are but temporary, historically accidental, and that by acting upon these conclusions we help to perpetuate the conditions which give rise to the present dualism. It may be that the data, the present state of affairs, are injuriously affected precisely because in the past we have acted upon just this split which discourages careful experimentation in personal and social matters. Had all potential scientists and explorers since the fourteenth century argued that there were insuperable obstacles to inquiry and judgments about matters of fact, the effect would have been to throttle such inquiries and thus to have preserved the conditions which gave rise to the viewpoint. Similarly, a consequence of holding and acting upon the theory that there is an inherent dualism between matters of fact and matters of value, that experimentation in social-personal affairs is either impossible or too dangerous, is to keep in existence the conditions which give this theory whatever plausibility it has.

Under present conditions we are thus by habit myopic and divided in vision. We fail to see that taken *en masse* facts and values are perhaps equally verifiable. For every experienced event in the whole gamut of events, whether physical, biological, psychological, social, or symbolistic, is experienced both as fact and as interest. And events of every kind vary all the way from the relatively simple, unchanging, accessible, separable, and controllable, on the one hand, to the extremely complex, dynamic, inacccessible, intertwined, and uncontrollable, on the other. Hence both facts and values vary from the rather definitely

verifiable to the probably unverifiable. At one end of the scale, the facts about and the worths of many chemical substances, mechanical tools, household arrangements, and medical procedures are for specific human needs rather definitely verifiable because the ends are definite and the means and their effects are clearly determinable and constant. At the other extreme, many formulations of fact about light, atoms, electricity, life, heredity, intelligence, personality, economic cycles, or government policies are so uncertain because the events—past, present, and future—are so complex and perhaps ultimately in large part inaccessible and uncontrollable. Likewise, what we truly wish to wish and what the values of alternative courses of action are in relation to our personal and social situation is usually so uncertain just because the facts are uncertain—the facts about the present situation: what it makes possible and what effects would actually follow.

Yet may we not, after all, place the laurels for highest degree of verifiability upon the brow of fact? For is not every value a step farther removed from determining event? Does not a changing and uniquely individual organism affect the value of every fact? Yes, at least the latter often is true. So let us give a wreath to fact. But wait! We must not forget that fact also is complexly mediated. And is not every valuing and every valuation an event, the existence and nature of which is even more difficult to study than it is to formulate and scrutinize value itself? Then must we give also a prize to value, especially to the more instrumental and less ultimate of its members. Let fact and value share the honors for second place, for neither is worthy of the first. Could all possible factual formulations be compared impartially with valuative ones, each probably would be seen to be, *en masse*, equally unverifiable.

Bradley Polytechnic Institute, Peoria, Ill.

Science and the Social Order[1]

BY

ROBERT K. MERTON

FORTY-THREE years ago Max Weber observed that "the belief in the value of scientific truth is not derived from nature but is a product of definite cultures."[2] We may now add: and this belief is readily transmuted into doubt or disbelief. The persistent development of science occurs only in societies of a certain order, subject to a peculiar complex of tacit presuppositions and institutional constraints. What is for us a normal phenomenon which demands no explanation and secures many 'self-evident' cultural values, has been in other times and still is in many places abnormal and infrequent. The continuity of science requires the active participation of interested and capable persons in scientific pursuits. This support of science is assured only by appropriate cultural conditions. It is, then, important to examine those controls which motivate scientific careers, which select and give prestige to certain scientific disciplines and reject or blur others. It will become evident that changes in institutional structure may curtail, modify or possibly prevent the pursuit of science.[3]

Hostility toward science may arise under at least two sets of

[1] Read at the American Sociological Society Conference, December 1937. The writer is indebted to Professor Read Bain, Professor Talcott Parsons, Dr. E. Y. Hartshorne and Dr. E. P. Hutchinson for their helpful suggestions.

[2] Max Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*, Tübingen, J. C. B. Mohr, 1922, 213; *cf.* P. A. Sorokin, *Social and Cultural Dynamics*, New York, American Book Co., 1937, esp. II, Chap. 2.

[3] *Cf.* R. K. Merton, *Science, Technology and Society in Seventeenth Century England*, Osiris History of Science Monographs, Bruges, 1938, Chap. XI.

conditions, although the concrete systems of values—humanitarian, economic, political, religious—upon which it is based may vary considerably. The first involves the logical, though not necessarily correct, conclusion that the results or methods of science are inimical to the satisfaction of certain values. The second consists largely of non-logical elements. It rests upon the feeling of incompatibility between the sentiments embodied in the scientific ethos and those found in other institutions. Whenever this feeling is challenged, it is rationalized. Both sets of conditions underlie, in varying degrees, the current revolts against science. It might be added that such reasoning and affective responses are also involved in the social approval of science. But in these instances science is thought to facilitate the achievement of approved ends, and basic cultural values are felt to be congruent with those of science rather than emotionally inconsistent with them. The concrete position of science in the modern world may be analyzed, then, as a resultant of two sets of contrary forces, approving and opposing science as a large-scale social activity.

Since the space at our disposal is limited, we must restrict our examination to a few conspicuous instances of the current re-evaluation of the social rôle of science, without implying that the anti-science movement is in any sense thus localized. Much of what is said here can probably be applied to the cases of other countries.[4]

The situation in Germany since 1933 illustrates the ways in which logical and non-logical processes converge to modify or curtail scientific activity. In part, the hampering of science is an unintended by-product of changes in political structure and nationalistic credo. In accordance with the dogma of 'race purity,' practically all persons who do not meet the politically imposed criteria of 'Aryan' ancestry and of avowed sympathy with Nazi aims have been eliminated from universities and

[4] This summary judgment will be tested in a monograph which the writer is preparing in collaboration with E. Y. Hartshorne. This study will deal with the place of science in the modern world in terms of the analysis introduced in this paper.

scientific institutes.[5] Since these 'outcastes' include a considerable number of eminent scientists, one indirect consequence of the racialist purge is the weakening of science in Germany.

Implicit in this racialism is a belief in race defilement through actual or symbolic contact.[6] Scientific research by those of unimpeachable 'Aryan' ancestry who collaborate with non-Aryans or who even accept their scientific theories is either restricted or proscribed. A new racial-political category has been introduced to include these incorrigible Aryans: the category of 'White Jews.' The most prominent member of this new race is the Nobel Prize physicist, Werner Heisenberg, who has persisted in his declaration that Einstein's theory of relativity constitutes an "obvious basis for further research."[7]

In these instances, the sentiments of national and racial purity have clearly prevailed over utilitarian rationality. The application of such criteria has led to a greater proportionate loss to the natural science and medical faculties in German universities than to the theological and juristic faculties, as E. Y. Hartshorne has found.[8] In contrast, utilitarian considerations are foremost when it comes to official policies concerning the directions to be followed by scientific research. Scientific work which promises direct practical benefit to the Nazi party or the Third Reich is

[5] See Chapter III of E. Y. Hartshorne, *The German Universities and National Socialism*, Cambridge, Harvard University Press, 1937, on the 'purge' of the universities; *cf. Volk und Werden*, 5, 1937, 320–1 which refers to some of the new requirements for the doctorate.

[6] This is one of many phases of the introduction of a caste system in Germany. As R. M. MacIver has observed, "The idea of defilement is common in every caste system." *Society*, New York, Farrar & Rinehart, 1937, 172.

[7] *Cf.* the official organ of the SS, the *Schwarze Korps*, July 15, 1937, 2. In this issue Johannes Stark, the president of the Physikalisch-Technischen Reichanstalt, urges elimination of such collaborations which still continue and protests the appointment of three university professors who have been 'disciples' of non-Aryans. See also Hartshorne, *op. cit.*, 112–3; Alfred Rosenberg, *Wesen, Grundsätze und Ziele der Nationalsozialistischen Deutschen Arbeiterpartei*, München: E. Boepple, 1933, 45 ff.; J. Stark, "Philipp Lenard als deutscher Naturforscher", *Nationalsozialistische Monatshefte*, 71 (1936), 106–111, where Heisenberg, Schrödinger, von Laue and Planck are castigated for not having divorced themselves from the 'Jewish physics' of Einstein.

[8] The data upon which this statement is based will be published shortly by Dr. Hartshorne.

to be fostered above all, and research funds are to be re-allocated in accordance with this policy.[9] The rector of Heidelberg University announces that "the question of the scientific significance (Wissenschaftlichkeit) of any knowledge is of quite secondary importance when compared with the question of its utility."[10]

The general tone of anti-intellectualism, with its depreciation of the theorist and its glorification of the man of action,[11] may have long-run rather than immediate bearing upon the place of science in Germany. For should these attitudes become fixed, the most gifted elements of the population may be expected to shun those intellectual disciplines which have thus become disreputable. Even now, effects of this anti-theoretical attitude can be detected in the allocation of academic interests in the German universities.[12]

It would be misleading to suggest that the new German State has completely repudiated science and intellect. The official attitudes toward science are clearly ambivalent and unstable.

[9] *Cf. Wissenschaft und Vierjahresplan*, Reden anlässlich der Kundgebung des NSD-Dozentenbundes, January 18, 1937; Hartshorne, *op. cit.*, 110 ff.; E. R. Jaensch, *Zur Neugestaltung des deutschen Studentums und der Hochschule*, Leipzig, J. A. Barth, 1937, esp. 57 ff. In the field of history, for example, Walter Frank, the director of the Reichsinstituts für Geschichte des neuen Deutschlands, "the first German scientific organization which has been created by the spirit of the national-socialistic revolution," testifies that he is the last person to forego sympathy for the study of ancient history, "even that of foreign peoples," but also points out that the funds previously granted the Archaeological Institute must be re-allocated to this new historical body which will "have the honor of writing the history of the National Socialist Revolution." See his *Zukunft und Nation*, Hamburg, Hanseatische Verlagsanstalt, 1935, esp. 30 ff.

[10] Ernst Krieck, *Nationalpolitische Erziehung*, Leipzig, Armanen Verlag, 1935 (19th Printing), 8.

[11] The Nazi theoretician, Alfred Baeumler, writes: "Wenn ein Student heute es ablehnt, sich der politischen Norm zu unterstellen, es z. B ablehnt, an einem Arbeits- oder Wehrsportlager teilzunehmen, weil er damit Zeit für sein Studium versäume, dann zeigt er damit, dass er nichts von dem begriffen hat, was um ihn geschieht. Seine Zeit kann er nur bei einem abstrakten, richtungslosen Studium versäumen." *Männerbund und Wissenschaft*, Berlin, Junker & Dünnhaupt, 1934, 153.

[12] Hartshorne, *op. cit.*, 106 ff.; *cf. Wissenschaft und Vierjahresplan*, *op. cit.*, 25–6, where it is stated that the present "breathing-spell in scientific productivity" is partly due to the fact that a considerable number of those who might have received scientific training have been recruited by the army. Although this is a dubious explanation of the present situation, a prolonged deflection of interest from theoretical science will probably produce a decline in scientific achievements.

(For this reason, any statements concerning science in contemporary Germany are made under correction.) On the one hand, the challenging scepticism of science interferes with the imposition of a new set of values which demand an unquestioning acquiescence. But the new dictatorships must recognize, as did Hobbes who also argued that the State must be all or nothing, that science is power. For military, economic and political reasons, theoretical science—to say nothing of its more respectable sibling, technology—cannot be safely discarded. Experience has shown that the most esoteric researches have found important applications. Unless utility and rationality are dismissed beyond recall, it cannot be forgotten that Clerk-Maxwell's speculations on the ether led Hertz to the discovery which culminated in the wireless. And indeed one Nazi spokesman remarks: "As the practice of today rests on the science of yesterday, so is the research of today the practice of tomorrow."[13] Emphasis on utility requires an unbanishable minimum of interest in science which can be enlisted in the service of the State and industry.[14] At the same time, this emphasis leads to a limitation of research in pure science.

An analysis of the rôle of science in the Nazi state uncovers the following elements and processes. The spread of domination by one segment of the social structure—the State—involves a demand for primary loyalty to it. Scientists, as well as all others, are called upon to relinquish adherence to all institutional norms which, in the opinion of political authorities, conflict with those of the State.[15] The norms of the scientific ethos must be sacrificed, insofar as they demand a repudiation of the politically imposed criteria of scientific validity or of scientific worth. The expansion of political control thus introduces conflicting loyalties. In this respect, the reactions of devout Catholics who

[13] Professor Thiessen in *Wissenschaft und Vierjahresplan*, *op. cit.*, 12.

[14] For example, chemistry is highly prized because of its practical importance. As Hitler put it, "we will carry on because we have the fanatic will to help ourselves and because in Germany we have the chemists and inventors who will fulfil our needs." Quoted in *Wissenschaft und Vierjahresplan*, *op. cit.*, 6; *et passim*.

[15] This is clearly put by Reichswissenschaftsminister Bernhard Rust, *Das nationalsozialistische Deutschland und die Wissenschaft*, Hamburg, Hanseatische Verlagsanstalt, 1936, 1–22, esp. 21.

resist the efforts of the political authority to redefine the social structure, to encroach upon the preserves which are traditionally those of religion, are of the same order as the resistance of the scientist. From the sociological point of view, the place of science in the totalitarian world is largely the same as that of all other institutions except the newly-dominant State. The basic change consists in placing science in a new social context where it appears to compete at times with loyalty to the state. Thus, cooperation with non-Aryans is redefined as a symbol of political disloyalty. In a liberal order, the limitation of science does not arise in this fashion. For in such structures, a substantial sphere of autonomy—varying in extent, to be sure—is enjoyed by non-political institutions.

The conflict between the totalitarian state and the scientist derives in part, then, from an incompatibility between the ethic of science and the new political code which is imposed upon all, irrespective of occupational creed. The ethos of science[16] involves the functionally necessary demand that theories or generalizations be evaluated in terms of their logical consistency and consonance with facts. The political ethic would introduce the hitherto irrelevant criteria of the race or political creed of the theorist.[17]

[16] Limitations of space forbid a thorough discussion of the concept of "ethos" in this connection. Suffice it to say that ethos refers to an emotionally toned complex of rules, prescriptions, mores, beliefs, values and presuppositions which are held to be binding upon the scientist. Some phases of this complex may be methodologically desirable, but observance of the rules is not dictated solely by methodological considerations. The ethos of science, as every other social code, is sustained by the sentiments of those to whom it applies. Transgression is curbed by internalized prohibitions and by disapproving emotional reactions which are mobilized by the supporters of the ethos. Once given an effective ethos of this type, resentment, scorn and other attitudes of antipathy operate almost automatically to stabilize the existing structure. This may be seen in the current resistance of scientists in Germany to marked modifications in the content of this ethos. The ethos may be thought of as the "cultural" as distinct from the "civilizational" component in science. *Cf.* the writer's "Civilization and Culture," *Sociology and Social Research*, 21, 1936, 103–113.

[17] *Cf.* Baeumler, *op. cit.*, 145. Also Krieck (*op. cit.*, 5–6), who states: "Nicht alles, was den Anspruch der Wissenschaftlichkeit erheben darf, liegt auf der gleichen Rang- und Wertebene; protestantische und katholische, französische und deutsche, germanische und jüdische, humanistische oder rassische Wissenschaft sind zunächst nur Möglichkeiten, noch nicht erfüllte oder gar gleichrangige Werte. Die Entscheidung über den Wert der Wissenschaft fällt aus ihrer 'Gegenwärtigkeit', aus dem Grad ihrer Fruchtbarkeit, ihrer geschichtsbildenden Kraft. . . ."

Modern science has considered the personal equation as a potential source of error and has evolved impersonal criteria for checking such error. It is now called upon to assert that certain scientists, because of their extra-scientific affiliations, are *a priori* incapable of anything but spurious and false theories. In some instances, scientists are required to accept the judgments of scientifically incompetent political leaders concerning *matters of science*. But such politically advisable tactics run counter to the institutionalized norms of science. These, however, are dismissed by the totalitarian state as 'liberalistic' or 'cosmopolitan' or 'bourgeois' prejudices,[18] inasmuch as they cannot be readily integrated with the campaign for an unquestioned political creed.

From a broader perspective, the conflict is a phase of institutional dynamics. Science, which has acquired a considerable degree of autonomy and has evolved an institutional complex which engages the allegiance of scientists, now has both its traditional autonomy and its rules of the game—its ethos, in short—challenged by an external authority. The sentiments embodied in the ethos of science—characterized by such terms as intellectual honesty, integrity, organized scepticism, disinterestedness, impersonality—are outraged by the set of new sentiments which the State would impose in the sphere of scientific research. With a shift from the previous structure where limited loci of power are vested in the several fields of human activity to a structure where there is one centralized locus of authority over all phases of behavior, the representatives of each sphere act to resist such changes and to preserve the original structure of pluralistic authority. Although it is customary to think of the scientist as a dispassionate, impersonal individual—and this is not inaccurate as far as his technical activity is concerned—it must be remembered that the scientist, in company with all other professional workers, has a large emotional investment in his way

[18] Thus, says Ernst Krieck: "In the future, one will no more adopt the fiction of an enfeebled neutrality in science than in law, economy, the State or public life generally. The method of science is indeed only a reflection of the method of government." *Nationalpolitische Erziehung*, 6. *Cf.* Baeumler, *op. cit.*, 152; Frank, *Zunft und Nation*, 10; and contrast with Max Weber's "prejudice" that "Politik gehört nicht in den Hörsaal."

of life, defined by the institutional norms which govern his activity. The social stability of science can be ensured only if adequate defences are set up against changes imposed from outside the scientific fraternity itself.

This process of preserving institutional integrity and resisting new definitions of social structure which may interfere with the autonomy of science finds expression in yet another direction. It is a basic assumption of modern science that scientific propositions "are invariant with respect to the individual" and group.[19] But in a completely politicalized society—where as one Nazi theorist put it, "the universal meaning of the political is recognized"[20]—this assumption is impugned. Scientific 'findings' are held to be merely the expression of race or class or nation.[21] Insofar as these doctrines percolate to the laity, they invite a general distrust of science and a depreciation of the prestige of the scientist, whose discoveries appear arbitrary and fickle. This variety of anti-intellectualism which threatens his social position is characteristically enough resisted by the scientist. On the ideological front as well, totalitarianism entails a conflict with the traditional assumptions of modern science.

One sentiment which is assimilated by the scientist from the very outset of his training pertains to the purity of science. Science must not suffer itself to become the handmaiden of theology or economy or state. The function of this sentiment is likewise to preserve the autonomy of science. For if such extra-scientific criteria of the value of science as presumable consonance with religious doctrines or economic utility or political appropriateness are adopted, science becomes acceptable only insofar as it meets these criteria. In other words, as the 'pure science

[19] H. Levy, *The Universe of Science*, New York, Century Co., 1933, 189.

[20] Baeumler, *Männerbund und Wissenschaft*, 152.

[21] It is of considerable interest that totalitarian theorists have adopted the radical relativistic doctrines of *Wissenssoziologie* as a political expedient for discrediting 'liberal' or 'bourgeois' or 'non-Aryan' science. An exit from this cul-de-sac is provided by positing an Archimedean point: the infallibility of *der Führer* and his *Volk*. (*Cf.* General Hermann Goering, *Germany Reborn*, London, Mathews & Marrot, 1934, 79) Politically effective variations of the 'relationism' of Karl Mannheim (e.g. *Ideology and Utopia*) have been used for propagandistic purposes by such Nazi theorists as Walter Frank, Krieck, Rust, and Rosenberg.

sentiment' is eliminated, science becomes subject to the direct control of other institutional agencies and its place in society becomes increasingly uncertain. The persistent repudiation by scientists of the application of utilitarian norms to their work has as its chief function the avoidance of this danger, which is particularly marked at the present time. A tacit recognition of this function may be the source of that possibly apocryphal toast at a dinner for scientists in Cambridge: To pure mathematics, and may it never be of any use to anybody!

The exaltation of pure science is thus seen to be a defence against the invasion of norms which limit directions of potential advance and threaten the stability and continuance of scientific research as a valued social activity. Of course, the technological criterion of scientific achievement has also a positive social function for science. The increasing comforts and conveniences deriving from technology and ultimately from science invite the social support of scientific research. They also testify to the integrity of the scientist, since abstract and difficult theories which cannot be understood or evaluated by the laity are presumably 'proved' in a fashion which can be understood by all, i.e., through their technological applications. Readiness to accept the authority of science rests, to a considerable extent, upon its daily demonstration of power. Were it not for such indirect demonstrations, the continued social support of that science which is intellectually incomprehensible to the public would hardly be nourished on faith alone.

At the same time, this stress upon the purity of science has had other consequences which threaten rather than preserve the social esteem of science. It is repeatedly urged that scientists should in their research ignore all considerations other than the advance of knowledge.[22] Attention is to be focussed exclusively

[22] For example, Pareto writes: "The quest for experimental uniformities is an end in itself." See a typical statement by George A. Lundberg. "It is not the business of a chemist who invents a high explosive to be influenced in his task by considerations as to whether his product will be used to blow up cathedrals or to build tunnels through the mountains. Nor is it the business of the social scientist in arriving at laws of group behavior to permit himself to be influenced by considerations of how his conclusions will

on the scientific significance of their work with no concern for the practical uses to which it may be put or for its social repercussions generally. The customary justification of this tenet—which is partly rooted in fact[23] and which, in any event, has definite social functions, as we have just seen—holds that failure to adhere to this injunction will encumber research by increasing the possibility of bias and error. But this *methodological* view overlooks the *social* results of such an attitude. The objective consequences of this attitude have furnished a further basis of revolt against science; an incipient revolt which is found in virtually every society where science has reached a high stage of development. Since the scientist does not or cannot control the direction in which his discoveries are applied, he becomes the subject of reproach and of more violent reactions insofar as these applications are disapproved by the agents of authority or by pressure groups. The antipathy toward the technological products is projected toward science itself. Thus, when newly discovered gases or explosives are applied as military instruments, chemistry as a whole is censured by those whose humanitarian sentiments are outraged. Science is held largely responsible for endowing those engines of human destruction which, it is said, may plunge our civilization into everlasting night and confusion. Or, to take another prominent instance, the rapid development of science and related technology has led to an implicitly anti-science movement by vested interests and by those whose sense of "economic justice" is offended. The eminent Sir Josiah Stamp and a host of less illustrious folk have proposed a moratorium on invention and discovery,[24] in order that man may have

coincide with existing notions, or what the effect of his findings on the social order will be." *Trends in American Sociology*, (edited by G. A. Lundberg, R. Bain and N. Anderson), New York, Harper, 1929, 404–5. Compare the remarks of Read Bain on the "Scientist as Citizen," *Social Forces*, 11, 1933, 412–15.

[23] A neurological justification of this view is to be found in E. D. Adrian's essay in *Factors Determining Human Behavior*, Harvard Tercentenary Publications, Cambridge, 1937, 9. "For discriminative behavior . . . there must be some interest: yet if there is too much the behavior will cease to be discriminative. Under intense emotional stress the behavior tends to conform to one of several stereotyped patterns."

[24] Of course, this does not constitute a movement opposed to science as such. Moreover, the destruction of machinery by labor and the suppression of inventions by capital

a breathing spell in which to adjust his social and economic structure to the constantly changing environment with which he is presented by the "embarrassing fecundity of technology." These proposals have received wide publicity in the press and have been urged with unslackened insistence before scientific bodies and governmental agencies.[25] The opposition comes equally from those representatives of labor who fear the loss of investment in skills which become obsolete before the flood of new technologies and from the ranks of those capitalists who object to the premature obsolescence of their machinery. Although these proposals probably will not be translated into action within the immediate future, they constitute one possible nucleus about which a revolt against science in general may materialize. It is largely immaterial whether these opinions which make science ultimately responsible for undesirable situations are valid or not. W. I. Thomas' sociological theorem—"If men define

---

have also occurred in the past. *Cf.* R. K. Merton, "Fluctuations in the Rate of Industrial Invention," *Quarterly Journal of Economics*, 49, 1935, 464 ff. But this movement mobilizes the opinion that science is to be held strictly accountable for its social effects. Sir Josiah Stamp's suggestion may be found in his address to the British Association for the Advancement of Science, Aberdeen, 6 Sept. 1934. Such moratoria have also been proposed by M. Caillaux (*cf.* John Strachey, *The Coming Struggle for Power*, New York, 1935, 183), by H. W. Sumners in the U. S. House of Representatives, and by many others. In terms of current humanitarian, social and economic criteria, some of the products of science are more pernicious than beneficial. This evaluation may destroy the rationale of scientific work. As one scientist pathetically put it: if the man of science must be apologetic for his work, I have wasted my life. *Cf. The Frustration of Science* (ed. by F. Soddy), New York, Norton, 1935, 42 *et passim.*

[25] Possibly because humanitarian sentiments are more deeply rooted among them or for other unascertained reasons, English scientists have especially reacted against the "prostitution of scientific effort to war purposes." Presidential addresses at annual meetings of the British Association for the Advancement of Science, frequent editorials and letters in *Nature* attest to this movement for "a new awareness of social responsibility among the rising generation of scientific workers." Sir Frederick Gowland Hopkins, Sir John Orr, Professor Soddy, Sir Daniel Hall, Dr. Julian Huxley, J. B. S. Haldane and Professor L. Hogben are among the leaders of the movement. See, for example, the letter signed by twenty-two scientists of Cambridge University urging a program for dissociating science from warfare (*Nature*, 137, 1936, 829). These attempts for concerted action by English scientists contrast sharply with the apathy of scientists in this country toward these questions. The bases of this contrast might profitably be investigated. In any event, although this movement may possibly derive from the sentiments, it may serve the function of eliminating one source of hostility toward science in democratic regimes.

situations as real, they are real in their consequences"—has been repeatedly verified.

In short, this basis for the re-evaluation of science derives from what I have called elsewhere the "imperious immediacy of interest."[26] Concern with the primary goal, the furtherance of knowledge, is coupled with a disregard of those consequences which lie outside the area of immediate interest, but these social results react so as to interfere with the original pursuits. Such behavior may be rational in the sense that it may be expected to lead to the satisfaction of the immediate interest. But it is irrational in the sense that it defeats other values which are not, at the moment, paramount but which are none the less an integral part of the social scale of values. Precisely because scientific research is not conducted in a social vacuum, its effects ramify into other spheres of value and interest. Insofar as these effects are deemed socially undesirable, science is charged with responsibility. The goods of science are no longer considered an unqualified blessing. Examined from this perspective, the tenet of pure science and disinterestedness has helped to prepare its own epitaph.

Battle lines are drawn in terms of the question: can a good tree bring forth evil fruit? Those who would cut down or stunt the tree of knowledge because of its accursed fruit are met with the claim that the evil fruit has been grafted on the good tree by the agents of state and economy. It may salve the conscience of the individual man of science to hold that an inadequate social structure has led to the perversion of his discoveries. But this will hardly satisfy an embittered opposition. Just as the *motives* of scientists may range from a passionate desire in the furtherance of knowledge to a profound interest in achieving personal fame and just as the *functions* of scientific research may vary from providing prestige-laden rationalizations of the existing order to enhancing our control of nature, so may the social *effects* of science be considered pernicious to society or result in the modification of the scientific ethos itself. There is a tendency for

[26] Robert K. Merton, "The Unanticipated Consequences of Purposive Social Action," *American Sociological Review*, 1, 1936, 894–904.

scientists to assume that the social effects of science *must* be beneficial in the long run. This article of faith performs the function of providing a rationale for scientific research, but it is manifestly not a statement of fact. It involves the confusion of truth and social utility which is characteristically found in the non-logical penumbra of science.

Another relevant phase of the connections between science and the social order has seldom been recognized. With the increasing complexity of scientific research, a long program of rigorous training is necessary to test or even to understand the new scientific findings. The modern scientist has necessarily subscribed to a cult of unintelligibility. There results an increasing gap between the scientist and the laity. The layman must take on faith the publicized statements about relativity or quanta or other such esoteric subjects. This he has readily done inasmuch as he has been repeatedly assured that the technologic achievements from which he has presumably benefited ultimately derive from such research. Nonetheless, he retains a certain suspicion of these bizarre theories. Popularized and frequently garbled versions of the new science stress those theories which seem to run counter to common sense. To the 'public mind,' science and esoteric terminology become indissolubly linked. The presumably scientific pronouncements of totalitarian spokesmen on race or economy or history are for the uninstructed laity of the same order as announcements concerning an expanding universe or wave mechanics. In both instances, the laity is in no position to understand these conceptions or to check their scientific validity and in both instances they may not be consistent with common sense. If anything, the myths of totalitarian theorists will seem more plausible and are certainly more comprehensible to the general public than accredited scientific theories, since they are closer to commonsense experience and cultural bias. Partly as a result of scientific advance, therefore, the population at large has become ripe for new mysticisms clothed in apparently scientific jargon. This promotes the success of propaganda generally. The borrowed authority of science becomes a powerful prestige symbol for unscientific doctrines.

Another feature of the scientific attitude is organized scepticism, which becomes, often enough, iconoclasm.[27] Science may seem to challenge the "comfortable power assumptions" of other institutions,[28] simply by subjecting them to detached scrutiny. Organized scepticism involves a latent questioning of certain bases of established routine, authority, vested procedures and the realm of the 'sacred' generally. It is true that, *logically*, to establish the empirical genesis of beliefs and values is not to deny their validity, but this is often the psychological effect on the naïve mind. Institutionalized symbols and values demand attitudes of loyalty, adherence and respect. Science which asks questions of fact concerning every phase of nature and society comes into psychological, not logical, conflict with other attitudes toward these same data which have been crystallized and frequently ritualized by other institutions. Most institutions demand unqualified faith; but the institution of science makes scepticism a virtue. Every institution involves, in this sense, a 'sacred area,' which is resistant to 'profane' examination in terms of scientific observation and logic. The institution of science itself involves emotional adherence to certain values. But whether it be the sacred sphere of political convictions or religious faith or economic rights, the scientific investigator does not conduct himself in the prescribed uncritical and ritualistic fashion. He does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed.[29]

It is this which in part lies at the root of revolts against the so-called intrusion of science into other spheres. In the past, this resistance has come for the most part from the church which

[27] Frank H. Knight, "Economic Psychology and the Value Problem," *Quarterly Journal of Economics*, 39, 1925, 372–409. The unsophisticated scientist, forgetting that scepticism is primarily a methodological canon, permits his scepticism to spill over into the area of value generally. The social functions of symbols are ignored and they are impugned as 'untrue'. Social utility and truth are once again confused.

[28] Charles E. Merriam, *Political Power*, New York, Whittlesey House, 1934, 82–3.

[29] For a general discussion of the sacred in these terms, see E. Durkheim, *The Elementary Forms of the Religious Life*, London, Allen & Unwin, n.d., 37 ff., *et passim*.

restrains the scientific examination of sanctified doctrines. Textual criticism of the Bible is still suspect. This resistance on the part of organized religion has become less significant as the locus of social power shifted to economic and political institutions which in their turn evidence an undisguised antagonism toward that generalized scepticism which is felt to challenge the bases of institutional stability. This opposition may exist quite apart from the introduction of certain scientific discoveries which appear to invalidate particular dogmas of church, economy and state. It is rather a diffuse, frequently vague, recognition that scepticism threatens the *status quo*. It must be emphasized again that there is no logical necessity for a conflict between scepticism, within the sphere of science, and the emotional adherences demanded by other institutions. But as a psychological derivative, this conflict invariably appears whenever science extends its research to new fields toward which there are institutionalized attitudes or whenever other institutions extend their area of control. In the totalitarian society, the centralization of institutional control is a source of opposition to science; in other structures, the extension of scientific research is of greater importance. Dictatorship organizes, centralizes and hence intensifies sources of revolt against science which in a liberal structure remain unorganized, diffuse and often latent.

In a liberal society, integration derives primarily from the body of cultural norms toward which human activity is oriented. In a dictatorial structure, integration is effected primarily by formal organization and centralization of social control. Readiness to accept this control is instilled by speeding up the process of infusing the body politic with new cultural values, by substituting high-pressure propaganda for the slower process of the diffuse inculcation of social standards. These differences in the mechanisms through which integration is typically effected permit a greater latitude for self-determination and autonomy to various institutions, including science, in the liberal than in the totalitarian structure. Through such rigorous organization, the dictatorial state so intensifies its control over non-political institu-

tions as to lead to a situation which is different in kind as well as degree. For example, reprisals against science can more easily find expression in the Nazi state than in America, where interests are not so organized as to enforce limitations upon science when these are deemed necessary. Incompatible sentiments must be insulated from one another or integrated with each other if there is to be social stability. But such insulation becomes virtually impossible when there exists centralized control under the aegis of any one sector of social life which imposes, and attempts to enforce, the obligation of adherence to its values and sentiments as a condition of continued existence. In liberal structures the absence of such centralization permits the necessary degree of insulation by guaranteeing to each sphere restricted rights of autonomy and thus enables the gradual integration of temporarily inconsistent elements.

The main conclusions of this paper may be briefly summarized. There exists a latent and active hostility toward science in many societies, although the extent of this antagonism cannot yet be established. The prestige which science has acquired within the last three centuries is so great that actions curtailing its scope or repudiating it in part are usually coupled with affirmation of the undisturbed integrity of science or "the rebirth of true science." These verbal respects to the pro-science sentiment are frequently at variance with the behavior of those who pay them. In part, the anti-science movement derives from the conflict between the ethos of science and of other social institutions. A corollary of this proposition is that contemporary revolts against science are *formally* similar to previous revolts, although the *concrete* sources are different. Conflict arises when the social effects of applying scientific knowledge are deemed undesirable, when the scientist's scepticism is directed toward the basic values of other institutions, when the expansion of political or religious or economic authority limits the autonomy of the scientist, when anti-intellectualism questions the value and integrity of science and when non-scientific criteria of eligibility for scientific research are introduced.

This paper does not purport to present a program for action in order to withstand the current threats to the development and autonomy of science. It may be suggested, however, that as long as the locus of social power resides in any one institution other than science and as long as scientists themselves are uncertain of their primary loyalty, their position becomes tenuous and uncertain.

Harvard University.

Science at the Tower of Babel

BY

T. SWANN HARDING

THE eleventh chapter of *Genesis* significantly begins to the following effect: "And the whole earth was of one language and of one speech." In that one language there was power. That power induced the inhabitants of earth to start the Tower of Babel with the idea of storming the citadel of Heaven. Whereupon, we learn, God confounded their language that they might not understand one another's speech. That confusion of tongues destroyed their power.

That confusion of tongues still exists. It occurs not only in everyday speech but in the speech of leading research workers. Here it impedes progress and creates difficulties. Perhaps Sir James M. Barrie had this in part in mind when he said that no one had anything worth saying these days except the scientists, and they did not know how to say it.

But more likely Sir James had reference to verbosity and circumlocutory English expression. That exists, it is true. Scientists do write poorly. They rarely use three short words to say anything if ten long ones will suffice. They do not merely keep the dust out of something; they "take adequate means to prevent the induction of minute particles from the atmosphere." They never simply begin a new experiment or investigation; they "initiate" or "inaugurate" it. They never conclude it; they bring it to a conclusion—and so on.

With such matters we are not greatly concerned herein. Scientific and medical journals have for years chided research writers for this sort of thing. They have jibed at them for saying

"instances of premature mortality are more frequent in case of females than in connection with males" instead of simply saying more women than men die prematurely. Such carelessness of expression does denote lack of an orderly mind. But the newer generation of scientific workers does write better than the old.

Moreover the scientist must use a certain small number of especially coined and defined words and symbols. This is because the sharp definitions of common words have been blurred by careless usage. But this is no license for unnecessarily inventing systems of words and notations and piling complexity upon complexity. That leads to confusion.

Oddly enough most ordinary people believe that scientists know exactly what they are talking about. When he wrote on words in Story magazine for December 1936, James Laughlin IV noted that the poet was always seeking to "disentangle words from their habitual associations in order to convey a personal perspective unadulterated by alien overtones." This is perhaps true.

But he continued: "The scientist has no similar problem, for science long ago broke with the common language as a carrier for its meaning and established for each field a peculiar system of signs and notation." That is probably much too true. For the delightful game of making up new words has been carried so far by the scientist that other scientists too rarely know what a specific writer or speaker means.

The necessity for precise definition in the field of science is paramount—but greatly neglected. Even when he uses one of the synthetic terms invented by scientists the individual research worker is very likely to be caught qualifying it by asserting that he does not mean by it precisely what soandso did.

Occasionally you will find a scientist with a genuine interest in definitions and who realizes that all science has its basis here. But even if he formulates a glossary it will be difficult for him to find a publisher and all but impossible to get other workers to agree with him. His colleagues will usually prefer to go on talking about scientific matters in strange, incomprehensible languages of their own.

In addition they will often use terms in such a way as to make them carry cargoes of personal emotion. Yet the technical terms of natural science are useful precisely to the extent to which they lack all aroma of approval or disapproval. C. K. Ogden and I. A. Richards, in their all but incomprehensible book, The Meaning of Meaning, list sixteen or more meanings of the word meaning itself.

But what does a word mean? To the physicist blue may mean light of an ascertainable wave length. To the poet it may mean moonlight or his beloved's eyes. To the ordinary run of us it means that color which we see when looking at a surface tinted the shade generally recognized by all normal people as blue.

The word orange is capable of any number of definitions, depending upon the type of person who defines. But for many purposes to say that it is the mature fruit of *Citrus sinensis* Osbeck best defines it.

But when a physicist, explaining Dirac's work, writes: "The value of a Cartesian coordinate of an electron at a particular time would be an observable," we should be very wrong to think that by the last word he denotes something that could be observed with a microscope. Not at all, though many physicists seem to think that. A knowledge of mathematics not of microscopy is necessary here. This "observable" is a mathematical abstraction, not an object or event. If the word were turned around and written *ebavresbo*, a cloud of habitual associations would be removed.

Here we have an instance where science should not have used a common word—at least without reversing it! That is because the word has such an aura of inferential meanings, and meaning is indeed a subtle thing as anyone can find by turning verse into prose. The effect and the very meaning of a prosaic statement differs greatly from that of patterned, cadenced verse. For meaning inheres in patterns, rhythms, and cadences as well as in stark definitions.

In conversation we can readily change the meanings of words by the inflection and intonation of our voices. This is why the same thing may be said effectively, with a smile, which, if written in a letter, would infuriate.

But it is a moot question how far we should go in this matter of inventing new words. Shall we, for instance, adopt the word "corylous" to denote the blue eyes of an immoral female black cat, or is it perhaps not important to have a special word for that? Might it not lead rather to confusion then to precision?

When Gertrude Stein writes about "tender buttons," or says that "a cloud is superior which is awake a satisfy sound," she presumably means something or other. But does her meaning not tend to become so indivualistic that she is almost alone in her understanding of it? That may be all very well in literature, but science must deal with definite propositions, expressed in precise language, and capable of verification.

Thus the statement "It is raining" can readily be verified. But, from the standpoint of science, the query: "Is the rain real, or does it exist only in my mind, or in the mind of God?" is nonsensical. There can be no answer verifiable by test.

When a doctor says that a certain person imagines she is sick or imagines she is in pain, he also is talking nonsense. It would be correct scientifically to say, however: "She is less sick than she thinks; she exaggerates her pain." The meaning of a scientific proposition depends upon the possibility of its verification. All else is poetry, emotion, or nonsense.

The unnecessary invention of technical nomenclature leads not to precision but to confusion. Our leading journal of chemical engineering not long since contained an editorial entitled "Don't Strut!" Therein the editor stressed "the desirability of writing even the most technical discussions in terms that would be easy for one not in that field to read and understand."

The editor deplored the present tendency in every specialty for "some workers to invent and devise new words and phrases possessing peculiar and exact meanings understood by a very limited group." Some of this language is not only uncalled for, it is inelegant jargon.

As long ago as 1927 the foremost British scientific weekly protested the highly specialized language used to report progress at a recent meeting of the British Association for the Advancement of Science. The papers were couched in language so formidable as to interest and be understood by few indeed of those

present, while they were all but unintelligible to the trained scientific correspondents of the press.

The presidents of the Association's various sections were accused of delivering addresses "calculated to produce in many members of the Association a feeling of bewilderment." This sort of thing, it was felt, tended to prolong the life of the popular superstition to the effect that "scientific experiment is mere legerdemain, its mysticism heightened by a rigmarole of complex terminology." All this deprives the public of its right to understand the ends and aims of science.

Medical journals in this country have also poured scorn upon the ambiguous and redundant "verbose paraphrasing" in medical writing. They have said that the use of polysyllabic terms has too often satisfied the need for real diagnosis and therefore confused medical thought. President Neilson of Smith College, addressing the Phi Beta Kappa Society remarked that the medical profession "has for a thousand years harbored practitioners who sought to gain prestige among the ignorant by using Greek and Latin terms for phenomena with perfectly good English names."

Scientists cannot attain prestige in national and world affairs until they attain clarity of expression. A minimum technical terminology may save time and avoid inaccuracy. But it is often, as President Neilson also said, "unnecessary hocus-pocus." Then he added that "today sociologists are building up a terrible jargon, though I have yet to find in their books an idea which is not capable of being explained in standard English." Horrendous language offers a formidable barrier to the dissemination and more widespread use of scientific knowledge.

It seems unnecessary to quote examples of technical or scientific language at its worst. In his English Rock Garden, Reginald Farrar has shown how a barrage of incomprehensible botanical terms can readily be translated into ordinary English fully intelligible to people of ordinary education and intelligence. Technical language readily lends itself to abuse for prestige purposes as President Neilson indicated. Quacks and charlatans know this well and therefore habitually speak and write in strange, incomprehensible tongues.

But, after reading pages and pages of it, one sometimes begins to wonder whether biologists have continually to write such sentences as: "Whenever two monosporidial mycelia meet within the leaf anastomoses form and a nuclear interchange takes place, followed by a localized diploidization and the development of a sōrus. Some hyphae are already diploid when they reach the sporogenous area of the sorus and can give rise to téliospores." And so on, and on, it seems interminably.

Possibly some profound thought exists here which must be embalmed in quaint polysyllabic verbiage. Or could the thought after all be of interest to the ordinary person if only expressed in ordinary English? We must, of course, not lose sight of the predicament in which Mark Twain found himself when in A Tramp Abroad he sought to explain how they hitch up horses in Europe, and to make no use of technical terms in so doing. He became involved in something like this:

"The man stands up the horses on each side of the thing that projects from the front end of the wagon, throws the gear on top the horses, and passes the thing that goes forward through a ring, and hauls it aft, and passes the other thing through the other ring and hauls it aft on the other side of the other horse, opposite to the first one, after crossing them and bringing the loose end back, and then buckles the other thing underneath the horse, and takes another thing and wraps it around the thing I spoke of before, and puts another thing over each horse's head, and puts the iron thing in his mouth, and brings the end of those things aft over his back. . . ." and also, so on and so on.

In December 1933 Prof. William Mansfield Clark delivered his presidential address to the American Bacteriological Society. He devoted considerable space to scientific word coinage. For scientists in each specialty already talked in totally different languages and seemed to have a sort of pride in making that specialty appear occult and esoteric. "A biologist passing from a meeting of biochemists to a meeting of cytologists might have heard discussions of the same subject in languages very different."

Few scientists, much less laymen, realize the importance of this. The coinage of new scientific words is estimated to rate

one a day. We are told that "man has always had a capacity for adding to his happiness or to his terrors by substituting long words for short ones." This should be very important to laymen. Our entire modern civilization depends upon scientific progress and insofar as verbal integration checks that progress civilization is imperiled.

Specialists not only invent new terms to satisfy their pride; they are reluctant to integrate their special terminologies with those used by workers in related subjects. Scientific correlation and cooperation are thus menaced. The fact that information about bacteriology, clinical medicine, and immunology is expressed in a special language for each offers a great obstacle to chemists, physicians, and bacteriologists who wish to be well informed.

Stuart Chase, with his customary readable, vivacious flippancy, is just now bringing to the attention of many laymen the fact that word symbols differ from realities and events and that we all tend to use poorly defined words. In his impetuous popularization of pioneers he is, as always, amusing. But the problem is serious and important. We are interested here in its more fundamental aspects.

It is general knowledge that writers of newspaper editorials, political quacks, and steaming hot reformers do not mean what they say. When they use words like Nazi, Communist, Fascist Democracy, Americanism, and the Constitution they are speaking emotively, not intellectually. The word is used as a challenge, in disapprobation or in approval. The Constitution may mean the original document and its amendments, or the interpretation given to the document by the courts over about a century and a half.

It is less generally appreciated, though still quite obvious, that when the justices of the Supreme Court write their opinions they too use words in an emotive, highly personal manner rather than always as carefully defined symbols. When a justice writes that "Just men regard repudiation and spoliation of citizens by their sovereign with abhorrence," he means by "just men"

a man like himself, and he is expressing a personal opinion in emotionally charged words.

What does he mean by "repudiation" and "spoliation?" Like the character in Alice in Wonderland the justice uses words to mean what he means them to mean. When a dissenting justice recently advocated "the free recognition that language, even of a Constitution, may mean what it says," he was setting up an illusive ideal. For the language of our Constitution habitually means something quite contradictory to two small groups of sincere old legal gentlemen.

These same bad habits extend to science. For a science is a congeries of definitions and, if these are ignored, nothing is possible. Many a conference of economists and sociologists has been thrown into disorder because no listener really knew what the speaker was talking about. Many a meeting of physicists and biologists has degenerated into an unseemly squabble simply because undefined terms were bandied about.

In economics there is as yet no generally agreed-upon definition of such commonplace words as "wealth," "money," and "income." There can be no possible agreement between two economists each of whom tacitly uses different, though nearly always unformulated, definitions of basic words. In his brief book, Brass Tacks, David Cushman Coyle lists a great number of commonly used undefined terms in this field, such as: Scarcity, technological unemployment, plenty, income, thrift, investment, seller's market, financing consumption and so on.

This same carelessness extends to biology. Protoplasm stands undefined, unless we accept the honest effort of a biologist who wrote of cytoplasm: "The name is simply a physical convenience to designate that particular area of the cell and the substances that fill it." Protoplasm is not something definite, unchanging. It is dynamic, not static. It is an interacting mixture of many structures and compounds in process of unending change.

Such words as heredity, inheritance, gene, and chromosome have many definitions, depending upon the speaker or writer. We have heard recently that virus particles are not living matter

but a crystalline protein. Yet when the protein is placed in contact with hospitable tissues it assumes some of the characteristics of living matter—it grows, reproduces, maintains itself, and mutates. What then is life? The word is used with quite different meanings by different biologists.

But surely physics is an exact science. Is it? Take a simple word like "temperature." What does it mean? Laymen and doctors say patients have a temperature when they are fevered. The patient may or may not feel hot. But if a clinical thermometer is thrust into his mouth it indicates a reading above that usually considered "normal" for a person in good health—though that normal reading is not absolutely exact. When we say the temperature is high we may really mean that the atmospheric humidity is excessive and oppressive.

When a chemist says he boiled water at one hundred degrees he is talking the language of Centigrade; a person accustomed to Fahrenheit or Reaumur or to absolute temperature-scale language might misunderstand. But the temperature of an electric furnace at high heat is measured not with a thermometer but with electrical apparatus. The temperature of a distant star is measured by calculations based upon the kind of light it emits. The temperature of the upper atmosphere may be estimated by calculating mathematically the radiant energy the ozone supposedly present there should absorb from the sunlight.

Thus temperature has many meanings. It is estimated by many procedures. They have different operational contents altogether. A substance may feel cool or cold to us whereas it may actually be as warm as its surroundings when temperature is measured instrumentally. If the light of a firefly were observed through an optical pyrometer it would appear that the insect was over a thousand degrees hot. Of course we can feel it and find out that is not so. But we cannot feel the distant stars usually observed by means of this instrument.

Is the upper atmosphere very hot or very cold? Certain physicists hold the former; others the latter. But, according to their own science, heat is caused by the rapid motion and constant collisions of the molecules composing a substance. Up high in the

outer atmosphere molecules are so rare that collisions must very seldom occur. Is it therefore scientific to speak of temperature, indicating heat or the lack of it, way up there? It probably is not, yet physicists do it just the same.

Physicists can be even more absurd. They speak of matter occurring in the form of a solid, a liquid, or a gas. But, if certain factors are chosen, it is possible for a physical scientist to prove that a gas is in reality a solid, just as he can prove that non-crystalline matter is really crystalline, assuming certain other factors. These things have been done. Serious books have also been written to demonstrate that all matter and force are forms of electricity. But the word "electricity" as used in such books bears little relation in meaning to the word you and your electrician use.

Did you ever stop to think how words mold science and make it what it is? The scientists who first described electricity as a "current" forever shaped science in this field. It then quite naturally began seriously to be assumed that electricity was something that flowed through wires as water flows between river banks. Naturally then it had a potential rate of flow influenced by the resistance it met. One term followed another and soon this cloud of symbols veiled the mystery of electricity and we felt that we completely understood it.

It was just a current or fluid. We also understood the atom when we could regard it as a tiny, infinitely hard, indivisible billiard ball; or later as a minute solar system; or later still as a particle of hardened electricity, or a wave motion, or a smoke ring. Then when mathematical symbols replaced those more easily pictured we felt that we had lost something real. The atom was gone. It had vanished. Actually we had never had it in the first place. It was a mental convenience at best.

We begin to see how carelessness about words impedes scientific progress. The print pages that exist to prove or disprove the existence of that often useful figment, the ether, would have been vastly less in number had everyone remembered that the ether of infinite space was a purely mental construct required in order to explain certain actually observed phenomena. In other words

rays of light came to us from afar *as if* such a thing as an ether existed which had just those properties required to transmit light.

But until scientists learn how to make themselves understood clearly by one another they will not go far towards making laymen understand them. And until they are generally understood it is futile to blame politicians for making too little use of science. How can they fully utilize something that not only they, but the scientists themselves, cannot understand because it is all so badly muddled with resistant language?

If words are used carelessly the whole structure of our industrial set-up tends to collapse. For pure research, which supports it then becomes practically impossible. Meanwhile education is impeded. Students waste time learning words that contribute nothing to precision of thought, while teachers try correctly to label each idea as it appears using the latest and most fashionable terminology. Long historical researches are necessary now to find the real distinction, if any, between latinized or greekified technical terms, whereas not even "rough" and "smooth" are well enough defined to be truly useful.

We have spoken of genes and atoms. Like electron, proton and ether these words refer to mental constructs with certain mathematical characteristics and other readymade properties. They are invented for convenience and are outfitted with precisely the qualities necessary. All scientists understand this at the start. But, very soon, an atom begins to seem quite as real as a fencepost or a brick. The economic man seems really to exist. The distinction is lost between word labels and realities. Serious workers in research begin to argue about words forgetting facts

Physicians begin gravely to state that "the character" of typhoid or scarlet fever or of smallpox is changing, or that syphilis and yaws become more like each other, or that influenza is just a form of encephalitis lethargica, or vice versa. They forget that these are mere word labels invented and especially defined for convenience. They are pigeon-holes into which certain groups of symptoms are slipped since they offer a ready means of classification.

But no such real entity as smallpox lurks about ready to pounce on us. To believe so would be like believing a personal

devil actually existed because we had invented the word "devil." That is a form of superstition, at least when we find it among primitive tribes. For we reserve the right not to believe in the existence of many entities for which primitives have names and in the existence of which they evidently believe.

George Washington died of a disease then called "cynache trachealis,"—literally "a dog strangled by the trachea." He was treated by being bled. Today physicians would doubtless call his ailment septic or streptococcic sore throat. It would be treated, as likely as not, in part by giving blood transfusions. The collection of symptoms has not changed. The name applied to that collection of symptoms and the method of treating them have both changed.

So also a term like "influenza" or "rheumatism" or "asthma" may be set up as a sort of miscellaneous label for what may well be several different ailments with distinct causes though the symptoms of those ailments overlap. Some cases of illness that were formerly classified und the label botulism or encephalitis lethargica may now be classified under influenza. That is all that can happen. Diseases themselves do not have characteristics which change.

In his Reason and Nature Morris R. Cohen drew attention to the fact that the very data of a science depend absolutely upon the terms used in its exposition. Hence, he said, the invention of new technical terms often created new facts as in social science economics, and psychiatry. People actually become introverts or neurotics because the terms are invented. So also when new terms appear in the International List of Causes of Death people begin obediently to die of the newly christened diseases.

Whenever a prominent medical specialist in any line affirms that one kind of disease of a certain organ is less common or less deadly than another, morbidity and mortality statistics soon reflect his pronouncement. Some years ago Sir James Mackenzie announced that myocardial affections were far more common than valvular affections of the heart; deaths from the latter promptly showed a sharp drop, while deaths from the former increased proportionately.

A physician, writing about this phenomenon, said that it

resulted largely "from the demands of 'style,' 'vogue,' or 'fashion in medical thought.'" The manner in which the statistics were tabulated also made a difference. "In other words the invention of new terms operates quite like the appearance of a new disease," said this doctor. This is very striking and remarkable, but is little considered. In practical terms it means that we have yet little sound reason to hold that the death rate from heart disease or from cancer is increasing.

It all depends upon how a death is classified, what cause of death is cited, and how accurate diagnoses are now and were some years ago. A man may have a combination of diseases when he dies. It is difficult to tell whether his death should be attributed to his diabetes, his heart ailment, an infection, or the fact that he fell out of bed. Also, for instance, the word "asthma" is, according to medical specialists, constantly applied to a wide variety of often quite unrelated pathological conditions. These are usually affections of the heart or lungs, but different ones.

How then would it be possible to tabulate statistically the incidence of asthma? It is not possible. In the field of psychiatry there exists what one physician has called "a new pseudo-neurological language." Vague, undefined terms abound in profusion to describe what laymen call nervous breakdowns. Specialists wisely speak of nervous instability, nervous disposition, neurasthenic temperament, nervous debility, nervous exhaustion, nervous breakdown, neurosis, psychosis—and on and on.

What do they mean? What is the difference between a neurotic, a psychotic, and a psychoneurotic—if any? How does a neurasthenic differ from a neuropath? What, on top of it all, is "neuritis?" Here is a term as ill-defined and as all-inclusive as rheumatism. What is meant by the cerebrocardiac neuropathy which has recently appeared from nowhere? In 1931 the National Conference on the Nomenclature of Disease tried to introduce a rational list of disease names to avoid the existing chaos in disease classification.

Our morbidity and mortality statistics are very erroneous and rational preventive medicine is impossible without an accurate

statistical background. A man is certified as dying from heart disease. It may well have been apoplexy. No postmortem may have been done. If an autopsy is done a cancer may be found, or kidney degeneration. The medical history may show a childhood attack of inflammatory rheumatism or a later venereal infection. Then what was the cause of death?

Again consider how many people who die of cancer are certified to have died from other causes in a quaint effort to save the feelings of relatives and friends. Once again—how many people did you ever hear tell of who actually died of syphilis? This disease slays its hundreds of thousands but, unless a person died in jail, in the poorhouse, or in an insane asylum, this disease is rarely mentioned even when it is outstandingly the cause of death. What do statistics amount to based upon such false reticence and verbal negligence?

On the other hand people have to die from one of the causes of death listed in the above-mentioned list. Yet some of the so-called "diseases of the heart" listed there are, like angina pectoris, merely symptoms that appear as a result of underlying ailments. But public health officials must use that list. They must often classify causes of death entirely indifferent to pathology or to basic causes of the diseased condition.

From such statistics the incidence of various diseases is calculated. From such lists conclusions regarding the relative healthfulness of certain communities are drawn. It is easy to see that such statistics can be no more reliable than their original source material. It is just as easy to see that, in spite of impressive statistics, we know little or nothing about the prevalence of various diseases simply because we use words too carelessly as disease labels.

Laymen have no idea how many unnecessary arguments are made and how many superfluous technical papers appear in print simply because scientists have been so negligent about their language that they do not know what they are talking about. A person like the present writer, who has spent nearly a decade reading, editing, and decoding research papers in manuscript grasps some notion of this.

As a single example among multitudes take vitamins. Not long since a worker in this field wrote to Science, our leading scientific news weekly, saying: "Vitamin B has turned into a full-sized swarm. Not only are there subscript numerals to the extent of six or more, but there are filtrate factors, X factors, and even cousins of the B swarm that fly around in the world of bacteriology under such names as Y. Not alone can B claim the honor of hatching a full-sized swarm, but D is a close rival. The last news indicated that there were about eight D's, and finally, lest we forget, such infants as H and K."

This scientist went on that each discoverer seemed quite innocent of the labors of his fellow workers because often the factors called by one letter have nothing in common but that letter! Most of this research has been done on rats. What about the results as applied to cows, pigeons, pigs, guinea pigs human beings, or bacteria? How about fleas, earthworms, and plants? What about the vitamins that hover under such vague descriptions as "evidence that a third factor exists" or "it appears that some further factor is necessary to ensure growth in the "widget?"

When several scientists break out at once with the discovery of different factors yet persist in applying the letter Q to them, what happens to progress? Pellagra is a disease of human beings. Goldberger apparently proved it was due to lack of a factor often called P-P, or pellagra-preventive, which occurred in yeast and in lean meat. It once seemed to be a fragment of vitamin B. This vitamin split into about twenty-two fractions—so far. A vitamin lettered G seemed to prevent pellagra.

But did it? Well, it prevented "vitamin G avitaminosis!" That is to say it prevented whatever happened when it was not there. But that work was done on rats. Was the disease pellagra then? Well, not exactly—for no rat fed a diet deficient in vitamin G ever developed symptoms such as pellagrins do. However, when dogs were placed on a diet deficient in this factor they developed blacktongue. This was described by some investigators as analagous to human pellagra. Then there is a dietary deficiency disease among chickens actually called chicken pellegra,—so what?

Careful scientists realize these distinctions. They hold them in mind. When they write they are careful to qualify or define their terms. But it is convenient to use a term without qualification. Less careful workers slip over into that. They write or say pellagra when they really mean blacktongue or some rat or chicken disease. They know what they mean. Their hearers or readers may not. Soon confusion supervenes and nobody knows what anybody means any more. Scientific progress goes to pot and articles appear based squarely upon the misunderstanding of words or symbols.

How rectify that? A constructive word in conclusion seems in order. Let there be some central clearing house of science where work in progress can be codified and indexed. Let this central clearing house freely inform workers throughout the country about what their colleagues are doing. Let it issue the right to use a certain symbol for ready reference to a certain factor with definite qualities, and let all workers conform. Then when somebody writes or says vitamin Q or factor Y everybody will know he means that stuff that acts soandso, just as we know what black is because we can check up by looking at it.

It is of the utmost importance to science that work begin here. All discussion of word usage in common life is futile until science carefully formulates a minimum number of technical terms so well defined that its workers will know what each other mean when this terminology is used. Scientific language must be made a practicable medium of communication.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

INDUCTION OF AN EXTERNAL WORLD

Dear Sir:

E. J. Nelson, in a recent article,[1] maintains that inductive reasoning can, in the nature of the case, furnish to an individual no evidence of "an external world—of whatever nature—mental, material, x-nature—which is not immediately experiencible." (p. 238)

The crucial point of his argument may perhaps be stated as follows: ". . . an hypothesis may be a valid explanation of given facts, only if it be finitely probable logically prior to these facts. And since in our problem these facts are the phenomenal world or events therein, the hypothesis of an external world must be finitely probable on evidence or knowledge of facts not included in our knowledge of the phenomenal world of phenomenal events." (p. 245) "This demand for a finite antecedent probability is deducible from the multiplication axiom of probability, which is, I suppose, as certain as anything we know about induction." (p. 241) And he concludes that it is impossible to establish any such antecedent probability for the existence of an external world "since all facts on which inductive investigations rest are themselves phenomenal." (p. 245)

D. C. Williams,[2] as well as many others, takes the opposite view. He believes that induction gives us overwhelming evidence of the reality of an external world. Like practically everyone else, he is fully convinced that the external world does exist—that his own indivial ego is not the whole of reality. I feel sure that in this Mr. Nelson agrees

[1] Phil. of Sci. 3, 237 (July, 1936).
[2] Monist 44, 80 (July, 1934).

with him. He believes that other persons exist and is interested in convincing them that he has no foundation for such a belief. Williams holds that it is impossible to acquire a belief in an external world by any act of direct knowledge, or by deduction from any such act. His only alternative therefore is that he acquires his belief by induction. He questions the ordinary assumption that an hypothesis with "antecedent finite probability" is a sine-qua-non of scientific induction.

C. D. Broad[3] takes a view intermediate between those of Nelson and Williams. He believes that induction gives us some degree of probability in reference to the external world, but he finds it difficult to account for the high degree of probability which he and all the rest of us believe in. He retains the view that an antecedently probable hypothesis is essential in the inductive process. He seeks, by means of an analogy drawn for the actions of machines, to establish an antecedent probability for the "Principle of Limited Variety" in nature; and this serves as a starting point for further induction. But it seems clear that no such analogy can be used to validate the fundamental inductive process of passing from subjective experience to a belief in an external world. Our concept "machine" is either a mental construct which has no external existence, in which case it could establish no probability about the external world; or it is a concept of an object which is a part of the external world, in which case it would involve cyclic reasoning to use it as a means of validating the inductive process by which we gain our belief in the existence of external reality.

We have then several alternatives to consider.

1st. No individual has any reason to believe in the existence of anything that is not a part of, or an experience of, his own ego. Our universal belief in the reality of other persons and other things is simply an illusion which is shared by all of us.

2nd. The individual reaches his belief in the reality of an external world by an act of direct knowledge, or by deduction from some such act.

3rd. The individual reaches his belief in an external world by the application of inductive reasoning to the data of his subjective experiences. In this case, *either*

(a) Scientific induction does not necessarily involve an "antecedent finite probability" of the reality of an external world, *or*

(b) Such a probability *is* available as a starting point for scientific induction.

[3] Proc. Arist. Soc. 28, 1, 1927–8.

The first alternative does not interest the serious-minded scientist. He classifies its underlying reasoning with that of the farmer who, after watching a giraffe for a time, turned to his wife and said, "There ain't no such beast."

Some have accepted the second alternative. But such an extension of the concept of reliable intuitive knowledge is rejected by most. It does not seem to agree with our experience, especially since science has made us acquainted with the intermediate steps between the activity of the other person or thing and our experience by which we become aware of them.

For most of us, then, the choice seems to be between (a) and (b) of the third alternative.

But first we ought to be very clear about what we mean by "antecedent finite probability." Its supposed necessity in scientific induction is derived from the mathematical laws of chance, or, what is equivalent for this purpose, the laws of equiprobability. These include the four axioms of equivalence, conjunction, disjunction and extension. From the law of conjunction is derived $h/c_1 \cdots c_n f = \frac{h/f}{c_1 \cdots c_n/f}$ (Equation I).[4] Scientifically interpreted, f represents known experiences in view of which the hypothesis h has a "finite probability"; $c_1 \cdots c_n$ are n independent implications of h, which have proved to be true, and which are not implied by f.

The mathematical concept of probability of a event is a ratio between two numbers, the numerator of which is the possible number of favorable cases, and the denominator the total possible number of cases. Equation I may be so interpreted, but when applied to scientific induction, the equation has a different meaning. When so applied, h/f no longer represents a mathematical ratio between two numbers, but an assumed hazy and indefinable relation between propositions. For scientific purposes, the equation means that the "probability" of any hypothesis h, which has a "finite probability" in view of the known facts f, is increased by an increase in the number of verifications of its independent predictions, and by the unexpectedness of the different predictions on the basis of f. Evidently the meaning of "probability" has here been changed, with the assumption that the conjunction axiom applies also to probability in this new sense.

For both kinds of probability h/f may lie anywhere between the limits 0 and 1, i.e., between impossibility and certainty. (In science, we call

[4] For proof see, for example, C. D. Broad, Mind, 27, 400, 1918.

an hypothesis probable only if h/f > 1/2, i.e., if it is more likely to be true than false. Scientific probability ranges from > 1/2 to 1, i.e., from a bare "probability" to certainty.)

This new kind of probability differs from mathematical probability in that it has no definite, assignable value, and in that h/f represents, not a fraction, but an assumed indefinable relation between propositions. Is it certain that the axiom of conjunction applies to this new kind of scientific probability? And if so, is it certain that when so applied h/f means anything more than the bare possibility involved in freedom from self-contradiction, and from inconsistency with any known fact; or that involved in the fact that, if true, it would "explain" the observed data, i.e., would be a general statement which includes the observed data as special cases? If this were all it means, then it would be difficult to see the force of Nelson's argument to prove that no "antecedent finite probability" for the existence of an external world can, in the nature of the case, be established.

When, as scientists, we stop to think just why we believe some scientific conclusion, is it certain that an antecedently probable hypothesis is a necessary part of the reason? Would it be possible for us to make a pure guess—straight out of the blue—with no possible reason for supposing that the guess was true, other than that it is not inconsistent with itself or with any known fact and that, if true, it would "explain" the observed data? If so, and if further experiment showed that a multitude of necessary implications of this guess were true, would this give us no feeling of probability that there was some truth in our guess?

Among the subjective experiences of the scientist, three groups are distinguishable:

Group I. Experiences which he calls voluntary, whatever that may mean.

Group II. Experiences which his experience convinces him have a necessary and dependable connection with those of group I. He is conscious, for example, of a dependable connection between a certain experience of willing, and a certain other subjective experience which he has come to associate with "muscular effort."

Group III. Experiences which, at first at least, he does not recognize as having any such connection with those of group I. (Psychologists now call these experiences "sense-impressions").

In the early childhood stages of the scientist's life of research, he gained, from Group I and II, the concept of dependable connection between events. Before long, the curious and unexpected variations in

his "sense-impressions" suggested to him the possibility that these "sense-impressions" might have some dependable connection with something outside of himself. We have come to call such dependable relation the "causal relation." But the scientist's guess that there was an external something that was influencing his "sense-impressions" did not necessarily involve the idea that the *nature* of this "cause" was the same as that of his voluntary experiences. It was only that there might be *some kind* of a dependable connection in the one case, as in the other. The *nature* of the "cause" was a matter for further research.

Let f represent all the data of Groups I, II and III. And let h represent our infant researcher's guess, his hypothesis, that there was something outside of him that was influencing his subjective experiences. In this case, did h/f represent an "antecedent finite probability," i.e., was $h/f > o$?

He had before him two mutually exclusive and collectively exhaustive alternatives, h and $\bar{h}$. Either such a connection did exist or it did not. It would seem, in view of all that f involves, that h was at least as probable as $\bar{h}$, i.e., that $h/f \not< 1/2$, and it was thus well fitted to serve as a starting point for the application of equation I. If so, this would enable him to accumulate a very large value for $h/c_1 - c_n/f$.

Would Keynes' argument that in such a case both h and $\bar{h}$ must have a antecedent finite probability invalidate such inductive reasoning by our scientist?

Our child scientist soon concluded, for example, that if his sense-impressions were really being influenced by something outside of him, then he might be able, by certain voluntary actions of his own, to modify the influence of this external something on his subjective experiences. He tried it, over and over again, and it "worked." He thus began building up his primary and secondary and tertiary and nth order hypotheses, and a great multitude of c's.

By applying this process in multitudinous and most complexly interwoven and pyramiding ways, he finally became confident of the reality of an external world, and of a causal relation between it and his sense-impressions, and he accumulated various beliefs as to things that were probably true about this external world, and he is still working at the job, and the end is not yet.

Wilmot V. Metcalf

College Hill, Clinton, N. Y.

REVIEWS AND NOTES

THE OLD GOD OF PHYSICS

Isaac Newton. By J. W. N. Sullivan. The Macmillan Company, New York, $2.50.

In this excellent biography, Sullivan's last work, Isaac Newton is presented not only as the greatest of scientific geniuses but as "one of the most singular and fascinating characters of which we have any record." The judgment is somewhat over-enthusiastic. Sullivan, with none too many facts at his disposal, makes it seem plausible.

Of the details of Newton's life we know little enough. From a few significant dates of his career, from several letters to friends and acquaintances, and from a very small collection of anecdotes it is possible to reconstruct the character by which Sullivan has been so greatly impressed. Newton was at different times generous, stubborn, absent-minded, tolerant, superstitious, and stupid. Almost all his life he was shy, and avoided society. But many another man has shown the same or similar qualities to an even greater extent, without having been considered particularly worthy of interest. The genius as a scientist, despite Sullivan's tendency to consider it as something quite distinct, is evidently an essential part of both Newton's singularity and his fascination.

As for this genius, there is here, but to a lesser extent than in most English biographies, a certain exaggeration. For instance, the distinction Newton makes between experimental fact and hypothesis is not something modern. It is a distinction born of the spirit of his own age, and the difference in this respect between Newton and his contemporaries is by no means so great as is supposed. Newton said, "I do not frame hypotheses," and proceeded to frame them. His contemporaries did little worse.

Newton himself was the first to point out that he stood on the shoulders of giants. It was in precisely those fields where the ground had been best prepared for him that he accomplished his greatest work. Like Leibnitz, he invented the calculus at a time when the ideas of

calculus were in the mathematical air. The theory of gravitation was not his alone. Knowledge of the paths of the planets was common property, and the inverse square law had been independently suggested by several men. What Newton added was tremendously difficult and important—but he *had* received help. In optics, where this help was lacking, all his brilliant experimental work failed to lead him to a fruitful theory. In chemistry, where until the middle of the eighteenth century all was confusion, his vast labors were thrown away. And in history and theology, to which he devoted most of his time, he accomplished less than nothing.

Sullivan is scrupulously fair to the various rivals who from time to time engaged Newton in controversy. The disputes have been pretty well settled, and are now unimportant except as they throw light on the characters of the persons involved. Newton came out of them with much credit. For one thing, he was honest. Most of his adversaries were not.

Sullivan's writing, as is to be expected, is beautifully clear. And the complete portrait he gives us of Newton is an admirable one. Here and there, as has been pointed out, there are slight distortions—but they are few, and far from serious. Newton was after all a giant, and if Sullivan does add an inch or two to his stature, the difference between Newton and ordinary mankind is hardly affected.

With regard to Sullivan's final judgment, there can be no cavilling. "The paradox of Newton's scientific career is due to the fact, probably unique in the history of scientific men, that he was a genius of the first order at something he did not consider to be of the first importance."

Joseph Samachson

New York.

A WORLD OF PROBABILITY

Experience and Prediction. By Hans Reichenbach. Chicago, University of Chicago Press, 1938. Pp. 410 $4.00.

The first half of the Twentieth Century will go down in history as the era of intellectual problems which steadily accumulated in science through the first three decades and, in 1929, were formulated for general reading in such books as John Dewey's "Quest for Certainty" and A. S. Eddington's "The Nature of the Physical World." Dr. Reichenbach's book, "Experience and Prediction" appears in the fourth decade to present an epistemology in which the nature of the physical world as it can be known by man is admittedly an uncertain quest. How a con-

fident book can be written in terms of uncertainty is admirably demonstrated in this work. Hume left no doubt that experience guided by prediction must be uncertain. Kant struggled manfully to regain certainty. It was inevitable that the scientific developments of the Twentieth century should compel someone to shun both the skepticism of Hume and the finality of Kant, and to present a constructive epistemology of uncertain knowledge.

Dr. Reichenbach undertakes this task with a proper background for it. Himself a pupil and colleague of Max Planck of Quantum fame, a philosophical interpreter of Einstein's theories (in various articles published in Germany from 1920 to 1928) he began his philosophical work originally as a critical Kantian, and has now emerged from this background as an independent leader in logistic empiricism with his interest centered on probability as the crucial factor in epistemology for philosophers, scientists and the common man alike.

His thesis in "Experience and Prediction," when stripped to the bone, is as follows: The hope of certainty has today centered on the immediacy of observation propositions. This is not justified. We actually observe neither external objects (Chapter II) nor our own impressions (Chapter III). We observe concreta, i.e. concreta arrive in each person's own immediate experience entirely without intellectual effort. From concreta, by mental effort, we infer the externality of objects and we infer impressions. All inference is predictional. Nothing predictional can be absolutely certain, for, if the prediction is realized it no longer predicts, and where there is no final, complete realization there is continuous prediction and lasting uncertainty. Therefore inferential knowledge, the only knowledge of objects and impressions which is possible, must be uncertain. At best, the necessity mental effort exercised on immediate experience leads only to probability. Success in knowledge must consist of giving relative probability weights to propositions. This all-important weighting process, briefly outlined in the concluding chapter, received its full development in an earlier major work by the same author, "Wahrscheinlichkeitslehre." (Leiden, 1935).

Judgment of "experience and Prediction" must be passed both on the psychological, epistemological soundness of the author's contentions about observation and inference, and on the logical soundness and practical utilizability of his proposed probability calculations and appraisals. Judgment on the former must be on the strength of this book; judgment on the latter must be on the strength of the previous book. Unfortunately, the two books are not in the same language, but the author,

who, in the fall of 1938, will join the philosophy faculty of the University of California at Los Angeles, has expressed his intention to republish his German work in English. The present book stands or falls with the earlier one. Although the pistemological convictions of the present work are the reason for a work on probability, the author realized that his epistemology would be self-defeating if he could not provide an adequate theory of probability as its modus operandi. For that reason he worked out the probability theory and, by publication, submitted it to criticism before he wrote the epistemology which it was intended to serve. Criticism of the probability theory must, therefore, be sought in reviews and published discussions of "Wahrscheinlichkeitslehre,"[1] and not in this review which must be devoted mostly to the theory unique to the present volume.

Criticism will center most strongly on the author's discussion of observation and impressions both because it is an innovation in psychological theory and because, if effective, it undermines the foundations of the current positivist doctrines. That concreta cannot be taken at their face value as external objects is made clear. If one observes by a roadside "a strange little man wearing a caftan" and, on closer observation, "he" turns out to be a wayside bush inference alone can tell which of these concreta is the external object. That concreta are not impressions is more difficult to argue. The intent of the argument is clear. Impressions sometimes occur without corresponding physical existents; by inference they must be due to something in the observer. So-called secondary qualities are produced from things only when those things enter into relations with our bodies. They are unobservable, inner relational processes somewhat analagous to the inner processes of an electric cell which can be inferred but not observed.

In criticism of the argument, it must be said that the complications of reinterpreted psychological terms become confusing. "Givens" and "concreta" and "things" are all both "observed" and "sensed," but "things," as observed or sensed, are claimed to be neither external objects nor internal states. Yet "an impression is my own internal state as it is produced by an indicated stimulus or a thing similar to that stimulus" (§ 20). In "having the inner process," (i.e. the impression) " 'having' does not mean 'given in observation.' . . . 'Having' denotes here only that the inner process occurred" (§ 26). But earlier (§ 19) it was said that we can "sense states of body" and to sense and to observe

[1] See especially Nagel's review of "Wahrscheinlichkeitslehre" in Mind XLV, No. 118, p. 501, and Dr. Reichenbach's reply in Philosophy of Science, Vol. 5, No. 1, January, 1938.

were used interchangeably. So it seems we observe inner states but do not observe inner processes, but, paradoxically, an impression is by definition an inner state and, as such, comes under the head of observable. Moreover the given is pronounced observable and "What is given are things, or states of things, including states of my body—not impressions" (§ 19) but, contrariwise, "An impression is my own internal state." (§ 20) All in all, it is difficult to keep conceptually clear the development of the author's initially clear assertion that concreta are observed but impressions are not observed.

Just what Dr. Reichenbach means by observation of concreta at first seems obvious as a simple experience but becomes progressively more obscure. If cleared up, it might shed light on how to apply his distinctions in the passages cited above. His avowed intent is always to employ meanings in accordance with science and the sociological fact of actual usage of terms. Nevertheless, he rejects any use which speaks of observing impressions. Since concreta which, admittedly we do observe, "enter into our knowledge without any intellectual operations being performed by us" (§ 25), this seems to credit us with the Lockean tabula rasa mind. The temptation would be to say that concreta enter *as* various groups or series of impressions. The author not only denies this but denies the Lockean power of reflection or observation of impressions, and, by insisting that *all* psychical phenomena are inferred, excludes observation from psychical as well as mental phenomena. Observation of concreta is therefore not a mental activity, it not a psychical phenomenon, and yet to observe concreta without mental activity is not to have impressions. The net result is that the reader finds observation of concreta an ultimate which is inexplicable but has tempting possibilities as a new basis of certainty from which inferences can be made and which thus serves the same purpose that experience of impressions does for the positivists. If not this interpretation, then the reader, in spite of clear warnings to the contrary, identifies "observation of concreta" and "having impressions" as one and the same phenonenon. Either outcome makes arguments in the other chapters appear like Don Quixote's tilts against a windmill, and is too incongruous a confusion to believe of a book remarkable, in general, for its logical lucidity. The thing to do seems to be to hope for further elucidation by the author in subsequent publications.

Extremely important in the influence it must have on modern trends in philosophy is Dr. Reichenbach's rebuttal of Positivism. If he successfully takes the certainty out of impressions, he leaves the Positivists

not a leg to stand on in their hope for knowledge which is certain, or in their dependence upon impressions as the basis of certainty. Moreover, he points out the positivist's error in supposed equivalence of indirect propositions and direct observation propositions,—impossible because the inferable direct propositions are an infinite class. That observation propositions cannot be certain is due to their predictional character, since prediction of persistent properties alone can distinguish a real from an illusory object. That impression propositions cannot be certain is the theme of Chapter III and part of Chapter IV with the author's arguments for the inferential character of impressions as discussed in this review.

Dependence on impressions results in an ego-centric language which the author carefully considers in logical comparison to realistic language. The justification of the realistic language,—in an important theory of what may be called inferential realism—makes Chapter II of great intrinsic worth. A projection theory of impressions is in important contrast to the positivist adherence to reducible relations, and it is also supremely necessary in an epistemology adequate to modern relativity and quantum science. Once the theory of projective complexes has been grasped as it can be only by reading the original statement, not by restatement in a review, its probability should be tested by Dr. Reichenbach's own technique of probability, for this will serve the dual purpose of giving a weight to his own theory and a test of the utilizability of his probability doctrine.

"Weight" is the key to Dr. Reichenbach's proposed practice since man in a world of moderate dimensions passes beyond the subjective limits of immediate experience by "the trestlework of probability connections." Complete uncertainty is not the alternative to certainty. Instead there are probabilities which weight the scales for decision to act in specific cases. The first step in this probability theory is to establish essential identity between the mathematical and logical concepts of probability. The difficulty is that the mathematical theory has not provided for prediction of individual instances, and this is what the logical concept demands for a "rational degree of expectation." To identify the two, probability is defined as "the limit of frequency" which assumes the convergence of frequency towards some stable ratio, with 1 as the extreme limit. One, in this case, need not mean certainty, but merely that all cases observed to date have been favorable cases. When the cases are in an infinite series and seem to defy convergence to a limit, the resort is to a "practical limit for a series showing a suffi-

cient convergence within a domain accessible to human observation" (§ 40). To apply this in daily life or to scientific laws necessitates appraisals of frequencies when an actual count is in no way possible. [It is interesting to note that any effort to determine probability without an exact numerical calculus is one which, Dr. Reichenbach's colleague in the University of Istanbul, the mathematician, R. v. Mises, pronounces impossible within any proper meaning of probability, and which he rigidly excludes in his book "Wahrscheinlichkeit, Statistik und Wahrheit." (Second edition, Julius Springer, Wien, 1936.)]

In Dr. Reichenbach's theory, even an appraised frequency determines the weight of judgment for or against a specific action for "a weight is what becomes of a degree of probability if it is applied to a single case" (§ 34). Dr. Reichenbach does not hesitate in the least to make a philosophy of gambling. His object is to find out how to gamble well. Every decision about a specific instance is a "posit" of a possible outcome based on the highest known probability for similar cases. The trick is to choose a class of cases to which the similarities are significant. In passing, it may be noted that if instinctive appraisals are admitted, it seems as though a factor in the weight might be wishful thinking, which the author does not discuss. Presumably, his reply to this would be that clear knowledge of the probability basis of decisions would be the best check on that tendency.

Of great importance and complication is the question of "concatenated inductions" when a needed probability involves interlocking inductive inferences from various sources all bearing on the case in point, with always, ultimately "blind posits on which the whole concatenation is based." It is points such as this which will send readers to the author's other works for fuller explanation.

Substantial points which will give psychologists, epistemologists and logicians fresh angles on old questions and new possibilities to consider are numerous. A few major points can only be listed here. The volitional element in knowledge is freshly clarified. A doctrine of "entailed decisions" as a criterion of choice between propositions otherwise weighted equally is highly significant and is employed repeatedly in the discussion. Super-empirical forms of knowledge, such as mysticism, are reduced to empirical knowledge plus emotive values, and, herewith, the author strikes a vulnerable point in the armor of super-empiricists but does not seem to drive it far enough home to be fatal; his victims are wounded but will probably continue to fight. The clear statements of technical, physical and logical meaning should be widely known and

used. The distinction between objective and subjective is put onto the statistical basis of greater frequencies of successful inferences from one type of experiences than from the other. The analysis of impression propositions into similarity disjunctions as effects of several possible alternative causes should be in current use. The problem of correspondence between stimulus-language accounts and reaction-language accounts of experience compels alliance between psychologists and logicians, and also sheds more light on the limitations of Behaviorism. These and many other points are integral parts of the whole thesis but, nevertheless, stand out as distinct issues in their own right.

Although the practise of science is the standard for all knowledge possibilities presented in this book the author no-where pretends to be able to explain the origin of scientific ideas; in fact he says, "the obscurity of the birth of great ideas will never be satisfactorily cleared up by psychological investigation." His concern is not with the context of discovery but with the context of justification. He insists the latter is the proper task of epistemology and thus leaves the former sadly orphaned in the asylum of obscurity. Many critics will object to such cursory disposal of this problem. The aim of this study of "Experience and Prediction" is to present predictive methods to steer the ship of knowledge safely between the phantom of absolute certainty and the shallows of complete ignorance. Guidance by principles of inductive inference may even lead to a posit of preference for non-inductive methods, but the basis of justification must not be forgotten, and its lead must be followed by whatever posits it weights most heavily. The author, confident in this guidance, says in conclusion, "To confess ignorance in the face of the future is the tragic duty of all scientific philosophy; but, if we are excluded from knowing true predictions, we can be glad that at least we know the road toward our best wagers."

In the opinion of this reviewer, certain features of this epistemology, as indicated, call for further clarification; but it is the most penetrating, conscientious attempt yet made to fathom the depths of uncertainty into which increasing knowledge anachronistically plunges humanity. It would be foolish hereafter to debate certainty or uncertainty without careful consideration of these arguments. The general quality of clear statement is akin in merit to Hume's philosophizing, and Dr. Reichenbach's views, in their turn, will prove disturbing to many philosophers' dogmatic slumbers.

Eleanor Bisbee

Amerikanj Koleji, Istanbul, Turkey.

BOOKS RECEIVED

The Kelley Statistical Tables. Compiled by Truman Lee Kelley. New York, The Macmillan Co. 136 pages. $4.50.

Shall we say that this is indispensable for biologists and psychologists? The computation of the derivatives and ordinates of normal distribution represents a heavy labor to satisfy an ever growing need. The contents are:

Table I. Eight-place normal distribution, simple correlation and probability functions.
Table II. Four-place χ^2 Functions.
Table III. Ten-place interpolation coefficients.
Table IV. Ten-place quintic interpolation coefficients.
Table V. Eleven-place septic interpolation coefficients.
Table VI. Eight-place square roots.
Table VII. Constants frequently needed.

Psychiatry. Journal of the Biology and the Pathology of Interpersonal Relations (Dedicated to the memory of William Alanson White). Volume One, Number one, February, 1938. Published by the William Alanson White Foundation, Washington, D. C. Quarterly, $6 yearly.

The topics covered in the first number include "anthropology and the psychiatrist," "antagonisms and business," "psychiatrists and political scientists," "basis and significance of homosexuality," "mental hygiene and the class structure," "an experience variable record," "the concept of tension in philosophy" and a chapter from a book on psychiatry. There are several short editorials, reviews and comments on periodical literature. The members of the publications committee are T. H. Gill, E. E. Hadley and H. S. Sullivan. "The journal is addressed not alone to psychiatrists and psychiatric research personnel in the narrower sense, but to all serious students of human living in any of its aspects."

Organon. International Review. II. Published by the Mianowski Institute for the Promotion of Science and Letters. Editor: Stanislaw Michalski. Warsaw, Staszic Palace, 1938. 302 pages. 15 zlotys.

The articles in this issue are: Science et Philosophie by Z. Zawirski, La Science et la Culture by C. Bialobrzeski, Investigation and Teaching by B. Suchodolski, Les Universités de France by S. Charlety, Visual

Thinking by A. Krogh. There is a set of "critical reviews" and an extensive questionnaire (100 pages) concerning the organization of a scientific congress. About 1/3 of the material is in English. The contributions are "middle course" as one would expect to appear in a publication from a country so situated as Poland.

Literature as Exploration. By Louise M. Rosenblatt for the Commission on Human Relation. New York, D. Appleton-Century Co. 340 pages. $2.25.

This is a publication of the Progressive Education Association. The first part of the book emphasizes the broad human implications of literature and analyzes the nature of literary experience. The second part of the book explains how the teacher can help lead the student toward a more spontaneous and better balanced response to literary works. The final part presents in compact form some of the basic concepts of the social sciences which are necessary to intelligent discussion of the human problems constantly encountered in literature and shows how an awareness of these ideas may emerge from the usual concern with literary works and types of literary form.

The Philosophy of the Act. By George H. Mead. Edited by C. W. Morris in collaboration with J. M. Brewster, A. M. Dunham and D. L. Miller. Chicago, The University of Chicago Press. 696 pages. $5.00.

This volume is composed almost entirely of Mead's unpublished writings. It will receive a special review in the next issue.